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Civil Engineering.

Memoir upon the Stability of Revetments, and of their Foundations. By M. PONCELET, *Chef de Bataillon du Génie. Translated from "No.* 13 *du Mémorial de l'Officier du Génie," by Captain* JOHN SANDERS, *Corps of Engineers.*

[CONTINUED FROM PAGE 225.]

Results of Experiments concerning the Friction and Cohesion of Masonry.

57. In the endeavor to elucidate the subject last referred to, we have embodied, in the following tables, the results of the experiments of Rondelet, Boistard, Morin, &c., upon the resistance of various materials to sliding, either when friction acts alone, or when it is replaced by the cohesion of mortars. It is well ascertained by the researches of M. Morin, that the friction of well dressed stones follows the same laws, of proportionality to the pressure, and of independence of the velocity, and of the extent of the surfaces in contact, as those of woods and metals; that it has the least value during the motion, and that this minimum value should be adopted in planning constructions subjected to shocks; that the resistance before the rupture of the equilibrium is actually due, either to friction alone, or to the mere cohesion of the mortar, according to their reciprocal preponderance; that is to say, it is either simply proportional to the pressure, whatever may be the extent of the surfaces in contact—or, again, simply in ratio with this extent, whatever may be the intensity of the pressure. In the experiments of M. Morin, the loads were not, however, carried far enough to permit the assertion that adhesion and co-

hesion are really independent of the pressure under which the solidification at first took place.

Table of the ratio of the friction to the pressure of various materials, at the moment of starting from a state of rest, and after a certain repose under that pressure.

Nature of Materials.	Ratio of the friction to the pressure
Experiments of M. Morin.	
Soft limestone, well dressed, upon soft limestone,	0.74
Hard limestone, do. upon do.	0.75
Brick, upon do.	0.67
Oak, (across the grain,) upon do.	0.63
Forged iron, upon do.	0.49
Hard limestone, well dressed, upon hard limestone,	0.70
Soft limestone, upon do.	0.75
Brick, upon do.	0.67
Oak, (across the grain,) upon do.	0.64
Forged iron, upon do.	0.42
Soft limestone, upon soft limestone, separated by a coat of fresh mortar, made with fine sand,	0.74
Experiments of others.	
Cut sandstone upon cut sandstone (Rennie),	0.71
Cut sandstone upon cut sandstone, with fresh mortar between, (Rennie,)	0.66
Granite, well dressed, upon granite, (Rennie,)	0.66
do. upon granite, with fresh mortar between, (Rennie,)	0.49
Hard limestone, polished, upon hard limestone, polished, (Rondelet,)	0.58
Hard limestone, cut, upon hard limestone, cut, (Boistard,)	0.78
Wooden box, upon a pavement, (Régnier,)	0.58
A block of stone, upon a bed of dry clay, (Lesbros,)	0.51
do. upon a bed of moist and soft clay,	0.34
do. upon a bed of moist clay, covered with coarse sand,	0.40

58. As to the proper resistance of mortar to sliding, the result of Morin's experiments presents some variations relative to the influence of the extent of the surfaces, and which he explains by observing that the setting of the mortar is as much more rapid and perfect, as this extent is less; but as, upon this point, these results disagree with those of M. Boistard, also given in the following table, it is better, for the present, to suspend all judgment thereupon.

Table of the resistance of various materials to sliding, arising from the adherence or cohesion of the cementing mortars.

Kind of Stone and Mortar.	Surface in square decimetres.	Length of setting in days.	Mean resistance per sq. decimetre.
Experiments of M. Boistard.			kil.
Cut limestone, upon cut limestone, with a mortar of fat lime and fine sand, set in the air,	1 to 2	17	66
	3 to 5	17	94
Same upon same, the mortar set under water,	4.7	487	12
Same upon same, with a mortar of lime and cement, set in the air,	1 to 2	17	32
	3 to 5	17	53
Same upon same, with a mortar of lime and cement, set under water,	4.7	487	110
Experiments of M. Morin.			
Soft limestone upon soft limestone, with a mortar composed of one part hydraulic lime and three of fine sand, set in the air,	1 to 2	83	180
	2 to 3	48	120
	do.	43	101
	4 to 6	48	100
	7 to 8	do.	94
Common bricks, united by same mortar, set in the air,	1.3	48	140
	2.6	do.	100
Soft limestone upon soft limestone, united by plaster,	2	48	220
	8	do.	280
Blue fossil limestone, well dressed, upon same, united by plaster.	2.5	48	110
	4.5	do.	200

59. If we should wish to make use of the numbers furnished by this last table, in calculating the thickness of the vertical wall, which has heretofore been considered by us (26 and 43); that is to say, in solely having regard to the cohesion of the mortar upon the foundation, we should obtain, on the hypothesis of sliding, the following equation:

$$\frac{\gamma x}{H} = \frac{\delta' p}{2}\left(\sqrt{1+f^2} - \sqrt{u^2+f^2}\right)^2 (1+a)^2$$

in which γ represents the force of cohesion for a square metre of surface, δ' a coefficient of stability properly chosen; p, f, a, and u, the same quantities as in number 1.

This equation, which, moreover, is of the fourth degree in x, on account of the expression of u (26), can be resolved and reduced into a table by precisely analogous proceedings to those in number 43; but we do not insist upon this solution, which is rather unusual, since it answers, in all cases of practical application, to neglect the cohesion of the mortar. We shall solely remark, that its value for unity in length of the wall being expressed by $\gamma e = \gamma H x$, while the weight of this

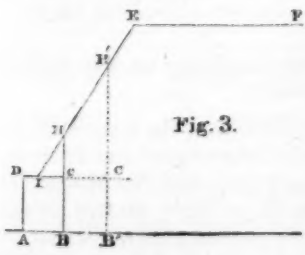


Fig. 3.

last, in supposing it vertical and covered with a prism of earth, ICH, (fig. 3,) is expressed by $\frac{1}{2} f p (x-m)^2 H^2 + p' H^2 x$; the cohesion will exceed the friction, when we shall have

$$\gamma H x > \tfrac{1}{2} f' f p (x-m)^2 H^2 + f' p' H^2 x,$$

or,

$$x < m + \frac{\gamma - f' p' H}{f f' p H}\left(1 + \sqrt{1 + \frac{2 f f' m p h}{\gamma - f' p' H}}\right)$$

which fixes in advance, independently of the height of the mass of earth BHEF, the limit of the thickness of the revetment within which this circumstance happens, for any designated height H of masonry, and width of berm ($b = m H$). Supposing, for example, $m = 0$, or a revetment without a berm, we shall have x, or

$$\frac{e}{H} < \frac{2\gamma}{f f' p H} - \frac{2 p'}{f p}, \text{ that is to say, } e + \frac{2 p'}{f p} H < \frac{2\gamma}{f f' p},$$

in order that the cohesion of the masonry should exceed its friction; so that it is only for low and thin revetments that it will exercise any influence in the case of sliding.

Besides, the consequences would be very different on the hypothesis of rotation, and of a firm and perfect setting of the mortar.

60. We shall now present the result of M. Morin's experiments relative to the friction of stone when in motion; the friction, which should alone be considered in the establishment of constructions subjected to any kind of shocks, or jars.

Character of the Plane Surfaces in contact.				Ratio of friction to pressure.
Soft limestone, well dressed,	upon same material,			0.64
Hard limestone,	upon soft limestone,			0.67
Common brick,	upon	do.		0.65
Oak, (across the grain,)	upon	do.		0.38
Wrought iron,	upon	do.		0.69
Hard limestone, well dressed,	upon hard limestone,			0.38
Soft limestone, well dressed,	upon	do.		0.65
Common brick,	upon	do.		0.60
Oak, (across the grain,)	upon	do.		0.38
Wrought iron, (lengthwise,)	upon	do.	(dry,)	0.24
The same, surfaces being wet,				0.30

Choice of a coefficient of stability relative to sliding on the beds of the courses of masonry.

61. This last table, and that of No. 57, show that the nature of the stones can exercise a great influence on the intensity of the friction; and we should on this ground, therefore, regret that M. Morin did not also make experiments on stones with surfaces merely rough hammered, as well as when perfectly dressed, and, above all, that he should not have examined the influence of a layer of fresh mortar.

We can, however, judge, from the numbers in the table of article 57, that, in general, this influence would be but slightly sensible for soft stones; whilst, on the contrary, it would be very appreciable for hard stones, which offer by themselves, without mortar, a certain resistance to sliding. But, as in the kind of constructions we are more likely to consider, the beds of the stones are far from being perfectly dressed, and as the mortar, without having attained its final hardness, nevertheless, to a certain degree, is always set, before being subjected to the horizontal action of the pressure; we should, therefore, admit, for the coefficient of friction, a superior value, in some cases, to that expressed by the figures in this last table, and probably we should be on the safe side, and still under the reality, in adopting 0.67, or even 0.70, as a coefficient for calculating the thickness of revetments on the hypothesis under consideration.

62. From this last consideration, if we consider, in particular, (55,) the number 2.65 as a proper value of the ratio $\delta' : f'$, in the case of ordinary earth and masonry, to reproduce approximately the thicknesses furnished by the profile of Vauban for demi-revetments; we shall be led to a coefficient of stability $\delta' = 2.65 \times 0.70 = 1.855$, very little below 1.912, the one which was obtained on the hypothesis of rotation (18). We would arrive at a still smaller value if we took f' below 0.70; which demonstrates that a similar manner of determining the necessary excess of stability for masonry to counteract the effects of sliding, offers much uncertainty, and that perhaps it would be more suitable to fix the value of δ', in each case, from the foreseen chances of injury and destruction to which the revetment might be subjected.

On this account, we think that we shall yield a great deal to stability in adopting, even for military constructions, the value $\delta' = 1.86$, in the case of sliding; and that it will suffice, under many circumstances, particularly where there are very great loads of earth, to take $\delta' = 1.5$, or even 1.4; which, in continuing to consider $f' = 0.7$ as the inferior limit of the coefficient of the friction of masonry, will give, at the most, $\delta' = 2f'$, $q = 1$, (51,) and will, consequently, lead to thicknesses always finite, and which, for great values of the ratio $p' : p$, will, as the calculation shows, constantly remain inferior to those which the hypothesis of rotation furnishes. In effect, in seeking the limit of x, the ratio of the thickness of the wall to its height, on the hypothesis in question, and for the different values attributed to the ratio $p' : p$, and to f, in the table of number 34, we shall, by a comparison of the results with the corresponding numbers of this table, obtain the following for comparison of their relative values:

Rotation,	1.337	1.175	2.144	1.541	1.243	0.927	0.461	0.934	0.789	1.279	0.769
Sliding,	3.193	2.770	2.358	1.524	1.000	0.337	0.333	0.901	0.501	0.458	0.206

63. The difficulty, or rather the uncertainty, which we here experience in fixing the value of the coefficient δ' from the example of the profile of Vauban, chiefly lies in this singular influence of the ratio

$\delta' : f'$, or of the number q, (47 and 51,) which tends to increase the thickness of revetments in a very rapid manner, and even to render them infinite, when we wish to secure a certain proportional degree of stability, whilst it is quite different in the case of strict equilibrium, or of a feeble stability, for either of which it would even be inferior to that which applies to the hypothesis of rotation.

In truth, the example of infinite loads never presents itself in practice; but, in order to assure a suitable degree of stability against sliding to a sustaining wall, such as ABCD (fig. 3), covered, for example, by a load of earth, IEF, twenty to thirty times its own height, it will be quite repugnant to any constructor to admit that it may be necessary, not only to double, or triple, but to centuple, the thickness which the calculation for the case of strict equilibrium, or $\delta' = 1$, would give, and that, while preserving the same hypotheses relative to the density and friction of earth and masonry. It is, however, the consequence to which they would inevitably arrive in the case under consideration.

To discover the cause of it, *a priori*, it suffices to consider, that, from the nature of the question, the ratio of the pressure of the earth to the weight of the wall and its superincumbent weight of earth, varies very little when the height of the earth is very great, even though there should be a considerable addition, such as BB′ for example, to the thickness of the masonry; and that it tends to converge slowly towards such a small number, that it cannot, in any case, exceed the ratio of δ' to f'; but this ratio of the pressure of the earth to the weight of the wall and its superincumbent load of earth, acquires a much greater value in the case of rotation, on account of the influence of the arm of the lever of pressure.

64. It follows, from this discussion, that, if we intend to maintain $f' = 0.7$ for the coefficient of the friction of masonry, and that, at the same time, we are unwilling to take 1.4, or even a slightly less number, for a coefficient of stability, it would be necessary, in the case of great height in the sustained embankment, to renounce entirely a like application of the calculation, which does not, besides, embrace many of the circumstances favorable to stability, and to seek to attain a proper economy in the masonry, by modifying the form itself of a profile thus become so disadvantageous in this extreme case; for example, by withdrawing, as Vauban often did, the foot of the exterior slope of the embankment, at least as far as the back edge of the top of the wall; a case for which the ratio of the thickness to the height, drawn from the very simple formulas of number 45, will always remain below unity, on the hypothesis under consideration; a value that it would hardly attain, if we took $\delta' = 2.65 f'$, $p' = p$, and $f = 0.6$.

65. This discussion gives rise to the reflection that, in the case of very high embankments, it does by no means follow that the bed of the base of the wall should be that which offers the relative minimum resistance to sliding, when the cohesion of the mortar is entirely neglected; which, in fact, is fully established by the table of number 53, which, as all the formulas of this chapter, remains applicable to

any horizontal course whatever, of which H indicates the distance below the top of the coping.

Supposing, for example, a demi-revetment without berm four metres high, with a parapet in earth of eight metres in elevation, counting from the coping to the middle of the superior slope, we shall find by this table, which is solely applicable to ordinary earth and masonry, on the hypothesis of $\delta' = 2.65 f'$; 1st, for the base of the wall, as $a = \frac{8}{4} = 2$, the thickness e (8th column) equal to $0.68\,H = 2.72$ metres; 2dly, for the course situated at 0.16 metres below the coping, where $a = \frac{8}{0.16} = 50$, $e = 17.35$, $H = 17.35 \times 0.16 = 2.776$ metres, a value materially greater than the preceding.

The same thing would, *a fortiori*, occur on the hypothesis of $\delta' = 3.3 f'$; but, as is seen, the remark only applies to the courses near the cordon of the wall; it is on the supposition that there is no berm, and that no consideration is taken of the effects of cohesion, which really plays a very important part for these courses.

Finally, the coefficient of stability δ', being at least considered equal (61) to 1.8, we would be led to different results, if we were willing to adopt a less stability, as the eleventh column of the table proves. This discussion shows the danger which there would, in some cases, be in filling earth behind a wall before the mortar had completely set. It, at the same time, explains the cause of the bulging out observed in certain cases, where walls, solidly retained at the ends, nevertheless yield to the action of the pressure of the earth, towards their summit, and about their middle.

66. On considering the result of the discussions and calculations which we have gone through in this chapter, and which appear thorough enough, we are of opinion that there is no cause to apprehend sliding on the beds of any of the courses of masonry, whenever the height of the sustained embankment shall not exceed four times that of the wall, or when, according to common usage, we shall have allowed the mortar in the masonry to have set completely before carrying up the embankment of the superincumbent earth. In the most uncertain cases, it will suffice, on the hypothesis $\delta' = 2.65 f'$, or even $\delta' = 2 f'$, to verify whether the values of m, x, a, &c., which answer to the profile adopted with reference to rotation, render the first member of equation (z) or (y) of number 43, superior to the second, for those courses near the top of the wall.

Besides, the question will be quite changed, as will be seen in Section III, if we take into consideration the sliding of the entire wall on the natural bottom of the foundation. As that which precedes is only applicable to vertical walls, it becomes necessary to show how we can always, by a suitable transformation of profiles, bring under the same head, questions relative to walls with the outer face inclined, or in batter, without recurring to new tables, or formulas.

(To be continued.)

FOR THE JOURNAL OF THE FRANKLIN INSTITUTE.

Railways in Massachusetts. By A. C. Morton, *Civil Engineer.*

The following tables exhibiting the cost of working, and the receipts, &c. of the principal railways of Massachusetts, for the year 1842, have been carefully compiled for the Journal, trusting they may afford some information of interest to its numerous readers.

They are intended to be a continuation of those published in the number of this Journal for December 1842; the authorship of which was by some mistake, ascribed to Alexander Evans, Civil Engineer.*

At the commencement of the year 1842, one of the most important of these roads (the Western road) was in an unfinished state and had only partially been opened for business. This and several others embraced in the tables have during the last year been extended.

Connexions have been formed with other roads in adjoining states, and large expenditures made with a view of increasing the facilities for doing a more extended business. For this reason the revenue of some of these roads when compared with the cost will not appear as favorable, as when their business relations shall have been fully established and all their improvements completed, which will enable them to do an increased business with greater celerity and economy. The West Stockbridge and Albany road, which is but an extension of the Western road, was not opened throughout the whole line until about the middle of September last; yet at the commencement of last year it was so far completed as, with the use of 15 miles of the Hudson and Berkshire road, to permit the running of the regular daily trains of the Western road over the whole line from the Worcester to the Hudson river, which in connexion with the Worcester road, completed the communication between the cities of Albany and Boston. The Albany and West Stockbridge Rail Road Company, by a special arrangement committed the construction of their road to the Western Rail Road Corporation, and also the sole use of the same, under a lease during the existence of its Charter, or a term of fifty years. The two roads are operated as one entire line between Worcester and Albany, and the business transacted under the name of the Western Rail Road Corporation.

As this great work has but just been completed, and during its progress has much attracted the attention of the public, I here give a brief sketch of its general features, and its first year's operations.

The distance from Worcester to the state line is $117\frac{6}{100}$ miles, thence to the Hudson river $38\frac{22}{100}$ miles, making the total length of road constructed by this company $156\frac{02}{100}$ miles. From Boston by the Worcester road to its intersection with the Western road, is 44.06 miles, making the whole distance from Boston to the Hudson river, $200\frac{08}{100}$, and to the Albany shore, $200\frac{35}{100}$ miles.

There are three summits on this road, viz: Carlton, between the Worcester and Connecticut river, which is elevated 906 feet above

* The tables referred to, were prepared in the early part of the year, but in consequence of being mislaid, were not forwarded for publication until the following autumn.

the grade of the Worcester road at the Mill Dam basin in Boston. Washington summit between the Connecticut and Hudson rivers, elevated 1456 feet above the same base, and Canaan summit between the state line and Greenbush, which is 954 feet. The depot at Worcester is 476 feet, that at Springfield nearly 71 feet, and that at Greenbush 26 feet, above the same plane of reference. The whole number of planes is 224, of which 40 are level, equalling $9\frac{4}{10}$ miles in extent. Of the greater inclinations of this road there are nearly 3 miles of 60 feet per mile, $1\frac{1}{2}$ miles of 68 and 69 feet, $5\frac{6}{10}$ miles of 74 feet, 6 miles of 78 and 79 feet, and 2 miles of $82\frac{1}{2}$ and 83 feet per mile. There are $75\frac{1}{2}$ miles, or 48 per cent of the whole road, curved to radii, varying from 22,920 to $859\frac{1}{2}$ feet. Of this distance, there are on the Western road, in Massachusetts, 10.6 miles of radii, between 882 and 1910 feet, 13.75 miles between 1910 and 2865 feet, 23 miles between the latter and 5730, and $7\frac{1}{2}$ miles over 5730 feet radius. The most abrupt curvature on this part of the road is encountered at each end of Tuttle Bend bridge, which curves are respectively of 955 and 882 feet radius. On that part of the road in New York, there are 20.45 miles varying from 6000 to $859\frac{1}{2}$ feet radius. The road-bed is graded for a single track, except a portion between Greenbush and the state line, embracing a distance of upwards of 23 miles, which with the deep cuts and high embankments on the whole road, are graded for a double track. The masonry is designed for a double track, with the exception of that for the Connecticut river bridge, and a few small structures. The total length of wooden superstructure of bridges is $1\frac{43}{100}$ miles; those east of Connecticut river are on the plan of Long's patent, and for a double track; all the remaining bridges are built after* Howe's patent, but, with few exceptions, designed for a single track.

There are 15 stone arched bridges, varying in spans from 10 to 60 feet, and in height above the several streams from 12 to 67 feet. At Canaan, near the state line, there is a tunnel of 548 feet in length, 26 feet wide, and 19 feet high. The sides and arch are supported by the rock through which it is cut. It contains 9920 cubic yards, and cost $35,000, or $\$3\frac{53}{100}$ per cubic yard.

A portion of this road has been very expensive; that part known as the Mountain Division, comprising a distance of about 14 miles, cost $980,000, or $70,000 per mile; and a single mile cost $219,929 The summit section of this division, $1\frac{8}{10}$ miles in length, contained 97,000 cubic yards of rock excavation, and the whole cost $241,312. The Connecticut river bridge, which is 1264 feet long, being 180 feet from centre to centre of piers, cost, including the casing of the trusses, the ornamental work, and the entire flooring covered with tin, $131,612. The engineer of the road stated that the cost of this bridge, when first brought into use, was, for foundations, masonry, and superstructure, each, about $40,000, or, for the whole, $120,000; since which, the cost of casing, &c., has been added.

The track of this road is formed of longitudinal sills, of 3 by 8

* For a drawing and description of Howe's plan of bridge, as applied on this road to pass the Connecticut river, see vol. iii, 3d series, page 289, of this Journal.

inches, upon which sleepers of chesnut, 7 feet long and 7 inches through, rest at distances of three feet from centre to centre. The edge rail of the T pattern is used, weighing 56½ lbs. per yard, which is supported by the sleepers. On a portion of the road, the sleepers nearest to the joints of the rails are 2½ feet from centre to centre. The length of turn-out and depot tracks is nearly 14¾ miles, giving a total of track laid by this company of 170¾ miles.

The total cost of the construction of this road, and its full equipment, with an allowance for some further expenditure, is $7,566,791. Relative to the business on this road, the last year, the following extracts from the last report of the directors of this company, will show some of the difficulties they encountered in the first year's operations of the whole line, and their views in relation to its future success:

"At the commencement of the year, the road had but just been partially opened to the Hudson river, giving access to a community before that time secluded from an eastern market, and to a business as yet unknown and untried; and this was accomplished only by the use of fifteen miles of another road, expensive in operation, and unfit for the kind of business important to our success; for the instability of the track of that road, and its high grades, required a great increase of power and of expense upon it. The business sought for had long been accustomed to other, and not undesirable, channels of communication, and to other and larger markets; and it required time and long-continued effort to divert it into a new direction. Everything was comparatively new to us; we had virtually no experience to guide in fixing upon tariffs of charges which would command the traffic; and no standard by which to judge of, much less to estimate with tolerable accuracy, the expenses of transportation, *on such a road.*

"The last year has been, therefore, emphatically, one of experiment—a year for ascertaining the difficulties of the trade in which we were embarked, and for gaining a knowledge of the means by which these might be obviated for the future. Notwithstanding these embarrassments, the undersigned look upon the past year as one of signal success. The *receipts* from the business have exceeded half a million of dollars, equal to more than $42,000 per month, and the expenditures have been but 67 cents per mile run."

The directors conclude their report as follows:

"Under these circumstances, the undersigned look *forward*, also, to the coming year, as one, comparatively, of great promise to the road, in all departments of its business; for they entertain strong hopes that their business will be greatly increased, and with a prospect of diminished expenditure in proportion to its amount."

The cost of transportation of freight on this road, the last year, has been greater than the previous year, which has been caused, partly, by the inequalities of the business in different months, but more particularly by the great preponderance of the trade eastward. The

amount of freight sent eastward from the Greenbush station, during the last year, was 30,688 tons.
Received at that station, during the same time, from the east, 5,624 "

Difference, 25,064 "

So great has been this excess of business eastward, that it occasionally became necessary to forward whole trains of empty cars from Boston to Greenbush, to meet the demand at that station. This was more particularly the case when the winter produce was seeking a market. The total amount of merchandise transported over this road, the last year, was equal to 6,211,971 tons *net* for one mile, or equal to, carried over the whole road, 156 miles, 39,820 tons. To do this business, the merchandise trains have run 160,089 miles, or equal to 1026 trips over the road, of 156 miles each. This gives 38¾ tons as the average tonnage per trip. Could the freight westward be increased, and more nearly equalized, the average load per trip would approach much nearer the capacity of the engines—consequently, with the same number of miles run, would do a much larger business, at but a small increase of expense. Great efforts have been made to accomplish this desirable object, and, towards the close of the year, with considerable success. The number of through passengers transported over the road was 18,570½, and way passengers, being for any less distance, 171,866, giving a total of 190,436½ during the year. The cost of running, the last year, has exceeded that of the previous year 1.6 cents; but the cost of repairs of cars and engines has been $1\frac{06}{100}$ cents less per mile run.

The total distance run on all the roads embraced in the tables is 1,502,029 miles, and the cost was $1,027,660, being $68\frac{4}{10}$ cents per mile, or 16 cents per mile less than the preceding year.

The cost of running, of repairs of engines and cars, and of road, per mile, is less, it will be seen by reference to table number 1, on the Norwich and Worcester road, than either of the others.

The manner in which this and the Western Railroad Company have exhibited and classified the expenditures for operating their roads the past year, cannot be too highly commended; yet, if these and other companies would extend this classification still further, so as to exhibit the cost of repairs of passenger and freight engines, the amount and cost of fuel for each, the repairs of passenger and freight cars, and all other expenses connected with the transportation of freight and passengers, each under separate heads, they would furnish much useful information to the public. The number of passengers and tons of freight transported should also be given.

The cost of repairs of cars and engines, it will be perceived, varies from 5⅓ to 20 cents per mile run. The excess in cost of repairs of engines and cars per mile on the Lowell road over others, may in part, perhaps, be attributed to the rigid and unyielding character of the road, it being constructed almost entirely of stone and iron. The average cost for repairs of engines and cars per mile run, for all these roads, is nearly 10 cents, or, exclusive of the highest two, it is $8\frac{41}{100}$

cents. The aggregate length of these roads (exclusive of the Boston and Maine road, the length of which is not known to the writer,) is 427¾ miles, and the total expenditure for road repairs is $188,820, or an average of $441 per mile. The cost of these repairs on the Boston and Worcester, and Lowell, roads, considerably exceeds that of previous years. The former of these has nearly the whole of the second track finished, or in progress of construction, and the latter has a double track complete.

Table number 2 shows the cost, receipts, expenses, and net income, of these roads. The cost includes all expenses for land, buildings, outfit, &c., or the amount charged to construction up to the close of 1842. The total cost of these roads is $20,976,688.

There has been an increase in the net proceeds of all these roads except three, the aggregate exceeding that of 1841 nearly $254,000. The decrease in the revenue of one of them was in consequence of a ruinous competition with rival lines. The difficulty has been so adjusted as probably to prevent its recurrence. On the other two roads, although the total receipts are considerably above those of 1841, the expenses were increased in a greater proportion, making a small reduction in the net revenue. The aggregate of the expenses of all these roads is 51 per cent. of the total receipts. The expenses are exclusive of interest paid on loans and debts.

Statement of the length, cost of construction, number of miles run, and the cost of repairs, of ten Railroads in Massachusetts, for 1842.—No. 1.

Name of Road.	Length of road.	Maximum grade per mile.	Minimum radius of curvature.	Weight of rail per yard in lbs.	Average cost per mile.	Miles run during 1842.	Cost of running per mile.	Cost of repairs of cars and engines, per mile run.	Cost of repairs of road per mile.
	miles	feet	feet				cts.	cts.	$
Boston and Worcester,	44.50	40	954	60	62,121	241,319	69.08	7.09	1156
Western,	156.14	83	882	56½	48,461	397,295	67.01	9.54	314
Norwich & Worcester,	58.09	20		56	36,647	144,321	51.59	6.66	116
Boston & Lowell	25.75	10	3000	56	76,826	143,607	91.23	20.06	1358
Boston & Providence,	41.17	37.50	5730	55	45,975	132.229	80.78*	10.21	512
Boston & Maine,				56		152,453	52	5.35	
Eastern,	56	40	2865	46½	41,053	184,127	61.25*		264
Nashua and Lowell,	14.25	13.70	900	57	26,666	44,040	99.50	16.25	260
New Bedford & Taunton,	20			58	21,306	40,734	57.33	10.84	172
Taunton Branch,	11			58	22,727	21,904	95.08	11.03	323

* This does not include expenses of ferry.

Statement of the receipts, expenses, and net income, of ten Railroads in Massachusetts, for 1842.—No. 2.

Name of Road.	Length of road.	Cost of construction.	Receipts for passengers.	Rec'ts for freight, mail, &c.	Total receipts.	Expenses of road.	Net income.	Per centage of income for expenses
Boston & Worcestr	44.50	2,764,396	186,610	162,596	349,206	168,509	180,697	48¼
Western,	156.14	7,566,791	266,447	246,241	512,688	266,619	246,069	52
Norwich & Worcester,	58.09	2,153,561	84,343	53,975	138,318	74,459	63,859	54
Boston & Lowell,	25.75	1,978,286	148,042	130,268	278,310	131,012	147,298	47
Boston & Providence,	41.17	1,892,831	163,788	72,680	236,468	112,824	123,644	47¾
Boston & Maine,		1,260,285	109,681	46,199	155,880	79,278	76,602	51
Eastern,	56	2,299,416	237,023	32,145	269,168	119,039	150,129	44¼
Nashua & Lowell,	14.25	380,000	66,305	64,883	131,188	91,577	39,611	70
New Bedford & Taunton,	20	426,122	43,483	12,292	55,775	23,354	32,421	42
Taunton Branch,	11	250,000	55,711	21,459	77,170	57,777	19,393	74¾

March 15, 1843.

Mr. Vignoles' Lectures on Civil Engineering, at the London University College.

[Continued from Page 241.]

LECTURE V.—ON THE COMPARATIVE ADVANTAGES OF DIFFERENT RAILWAYS.

The class will, no doubt, be inclined to think that I have dwelt too long, in the first four lectures of the present course, upon the principles of economy in motive power; but I assure you, that if, in after years, any of you follow up the profession, you will find the subject one of the most vital importance. I shall this evening draw your attention to the different elements of comparison which should guide the engineer in forming a selection from different proposed lines of railway, and shall take, as a text-book for that purpose, Mr. M'Neil's translation of M. Navier's work, *On the Means of comparing the respective Advantages of different Lines of Railway*—a work which I highly recommend for your private study, on account of the clearness and accuracy of the views it contains. M. Navier states "that the elements of comparison of different lines of railway may be divided into two heads; first, the establishment of a very rapid mode of transport—a consideration which should give a preference to the shortest lines, the velocity being supposed to be the same in all; second, the increase of wealth which may result from the establishment of a line of railway. The construction of a railway, like that of a common road, or a canal, is favorable to the advancement of wealth; in the first place, because the actual expense of transport in this di-

rection is diminished; and, in the second place, because this diminution in the cost of transport increases the value of the neighboring properties, facilitates the establishment of new works, and increases production;" and the saving effected is not merely a private advantage to those individuals who may be directly benefited by it, but is so much actual increase of the wealth of the country at large. "The first of these effects—that is to say, the diminution obtained on the actual cost of transport—is the cause of the second, so that this diminution is the principal circumstance, and that which should be principally considered." Taking it as established, therefore, that diminution in the cost of transport is the principal thing, we come to the result that the cost of motive power, on which this is dependent, is the leading point to be attended to in the formation of any line of railway. Indeed, M. Navier goes so far as to say that this is almost the only circumstance to be attended to; in his own words, "we should even say that the rate of reduction which is obtained upon the actual cost of transport, by the establishment of a new communication, is almost the only circumstance which should be thought of;" but he goes on to say, very justly, "it is also necessary to consider the quantity of goods which is carried, or which may be carried hereafter, in this direction," for the very essence of the railway system is to increase its own traffic; "for it is evident that it may be less advantageous to the country to produce a great economy in the cost of transport upon a line where there is but little to carry, and more advantageous to produce a less economy upon a line where a large quantity of merchandise is carried." These are the principles which I have been endeavoring to impress upon your minds, and which, from their importance, I cannot too often repeat. "It is, therefore," says M. Navier, "generally necessary to take into consideration, in the comparison of different lines, the quantity of traffic which may be established on each, and even the increase in the value of properties, and the development of production to which the establishment of these lines may give rise respectively, according to the nature of the countries which they traverse." I would observe, as a passing remark, that the word *developpement*, in French, generally refers to length; thus the development of a line of railway will be spoken of—meaning the length of that line—whilst, in English, the word refers to an extension of superficies. M. Navier does not go minutely into the examination of these last elements of the question, which rather belong to statistics and political economy than to engineering, but confines himself to the "consideration of the reduction which the establishment of a railway can effect upon the actual cost of transport—a most important consideration—to which, as already remarked, it is always necessary to attend; and this will form, in every case, the principal element of the comparison between different lines, and often leads to determinations purely geometrical or mechanical, and, consequently, exempt from arbitrary deductions."

M. Navier then goes on to state, that "the cost of transport on a railway, as upon a road, or canal, depends on two principal points, which it is necessary to distinguish and consider separately; the first

of these is the expense of constructing the railway, and the second is the expense of conveying the goods on the railway, when it is constructed. The expense of the construction of the railway is independent of the quantity of merchandise and of passengers that will pass over it. The expense of transport, properly speaking, upon the railway supposed to be constructed, depends, on the contrary, upon the quantity of merchandise or of passengers—that is to say, all other things being equal, the expense will evidently be in proportion to the tonnage." Now, a few years back, the whole time of the House of Commons was taken up with comparing the merits of rival lines of railway, for no sooner was one line proposed than directly a rival line was started. It is well known that, for the Brighton Railway, four different lines were proposed, the discussion on the respective merits of which extended over a considerable length of time. But it is a curious fact, that, in all these discussions, the principle which has been laid down this evening was never once alluded to. Now, in the practical working of railways, the diminution of expense of transport is generally quite independent of the quantity of goods carried; for, after a line is constructed, the charges are generally arranged with reference to rival lines, or to the competition which may exist with the railway; and the interest of the money laid out is scarcely thought of, however much it may have entered into the *a priori* calculations. The Paris and Versailles Railways may be mentioned; two lines were started, one on each side of the river—the government did not like to treat either party harshly, and passed both bills, and both lines are actually executed; and, from the great competition between them, the charges for transport of goods and passengers will probably bear little, or no, relation to the interest of the capital expended. There is, however, another element which renders the calculation of a very complicated nature. The railways are different from common roads, or canals, over which, after they have been once constructed, the public have been left to find their own way—considerations of public safety render it necessary to incur great expenses in terminal and local stations, &c.; and there are also secondary expenses, such as the annual cost of repairs, police, and management, of which it may be said that they depend partly on the interest of the cost of constructing, and partly on the amount of tonnage carried. Now, from experience a general idea can be formed of the expense of these items, but, before going into the details, I will return to M. Navier, who says,—"We may, therefore, admit, without falling into any serious error, that the annual cost of transport on a railway is, in all cases, formed of two parts—the one proportional to the expenses of the construction of the way, and the other proportional to the amount of tonnage carried; and we should also observe, that the cost of transport of one ton of merchandise cannot be specified, unless the number of tons which shall be carried annually from one extremity of the line to the other, be known." Now, hitherto we have been unable to determine *a priori* what these amounts are; but we can tell with great accuracy what they have been on the different lines of railway

now in operation. The following tables give the average of these expenses on several lines of railway:

Merchandise Traffic.

Heads of charge.	Coal on colliery railways in the north.	Goods on the Liverpool and Manchester Railway.
Locomotive power—wages and repairs,	0.355*	0.425*
" fuel,	0.025	0.125
Total,	0.380	0.550
Wagons,	0.190	0.227
Conducting traffic,	0.075	1.080
Maintaining railway,	0.208	0.307
General expenses,	0.100	0.354
Total cost,	0.953	2.518

* Per ton per mile—in decimals of a penny.

Passenger Traffic.

Heads of charge.	Lond. & Manch'r Railway—average 60 passengers per train.	Dublin & Kingstown Railway—av. 40 passengers per train.
Locomotive power—wages and repairs,	0.170*	0.173*
" fuel,	0.100	0.115
Total,	0.270	0.288
Coaches,	0.054	0.031
Conducting coaching,	0.104	0.113
Maintaining railway,	0.085	0.050
General expenses,	0.091	0.174
Total cost,	0.604	0.656

* Per passenger per mile—in decimals of a penny.

Taking the Liverpool and Manchester Railway as an example, we find the number of passengers to average sixty per train. This may, on the whole, be considered as a fair average on all the railroads throughout the country. Seven years' working of the same railway gives, as the average expense of locomotive power, 0.27*d.*, or about ¼*d.*, per passenger per mile. The gradients do not exceed six or seven feet per mile, with the exception of the inclined plane, and this also is an average amount for most railways—in fact, fuel and wages are so nearly the same on all lines, that the expense of this head can be calculated with great exactness. The expense of locomotive power, also, is the only one which depends upon the gradients. The other expenses, which are independent of the gradients, are—coaching,

conducting ditto, maintaining way, and general expenses, altogether amounting to 0.33*d.*, which, added to 0.27*d.* = 0.60*d.*, or, in round numbers, three-fifths of a penny per passenger per mile for the expense of transport. Now, let us examine the relative expense of the merchandise traffic. We have, for the expense of locomotive power, 0.55*d.*, or, in round numbers, ½*d.* per ton per mile; for the cost of wagons and secondary expenses, 1.97*d.*, which, added to 0.55*d.*, gives 2.52*d.*, or, in round numbers, 2½*d.* per ton per mile, as the actual cost of transport. Now, let us mark the very striking result of this comparison. Even with all the most recent improvements, and cutting down every expense that can be reduced, the mere transport of passengers costs three-fifths of a penny per passenger per mile, whilst that of goods is only 2½*d.* per ton for the same distance; and of this 1*d.* may be thrown out, arising from other sources, leaving the cost of transport—passengers, three-fifths of a penny per passenger per mile; goods, 1½*d.* per ton per mile. In the first case, we have an amount exceedingly high, in proportion to the present means of transport, whilst the second case presents a result as strikingly low. A ton of goods is equivalent to the weight of fourteen passengers, with 30 lbs. of luggage each.

When the loads to be carried are light, and the velocities at which they are carried considerable, the steepness of the gradients is a matter of comparatively little consequence; but as soon as the engine is loaded to its maximum power, the railway system becomes unable to compete with the canals, so far as relates to the carriage of goods. If these are the results offered to you by past experience, do you not see at once how it affects the question of laying out lines in remote districts, where but a small amount of traffic can be calculated upon? Again, referring to the table, with reference to the difference between carrying slowly and carrying quickly, we find that the expense of locomotive power on the Liverpool and Manchester is 0.55*d.*, or nearly three-fifths of a penny; yet that the expense upon the best railways, where goods are carried at a moderate velocity, is only 0.38*d.*, and the remaining expenses 0.57*d.*, so that it comes to this, that we have —Liverpool and Manchester Railway, 2½*d.* per ton per mile; other railways, with moderate speeds, 1*d.* per ton per mile. M. Navier proposes a case not quite so strong, perhaps, as might be made out, and I will, therefore, refer to the Brighton Railroad for an example, the expense of which, for the 40 miles, has been about £2,600,000, or £60,000 per mile, the interest of which, at 6 per cent., is 10*l.* per mile per day, which is the net receipt, after all expenses are paid, requisite to insure a decent interest to the shareholders. I shall not enter further into the question now; but if those students who are sufficiently advanced will take up the subject, they will soon be able to appreciate my arguments for increasing the limits within which gradients are usually kept—for, supposing the expense of carrying a passenger should be only ¾*d.* per mile, yet, if you will calculate the additional expense of the interest of £60,000 per mile, you will find ruinous results.

M. Navier having said that the cost of transport is the chief point

to be attended to in laying out a railway, goes on to determine the amount of power requisite to draw a given train over a given railway. The elder students will, in connexion with this subject, be aware of the opinion which has been generally entertained amongst engineers, that a rise of twenty feet per mile is equivalent to a mile in length. M. Navier says—"Let us observe that, upon a horizontal line, the power required to draw a given weight is considered as being equal to almost the two-hundredth part of this weight;" but, as I have shown in a previous lecture, the formula for the expression of this power will be $\frac{F}{n}$, taking F as the friction per ton, and n the number of pounds in each ton,—so that what M. Navier calls the two-hundredth part of the weight, will be friction divided by the number of pounds in a ton. Taking the friction at 9 lb., we have $\frac{9}{2240} = \frac{1}{249}$ nearly. At 11 lb., $\frac{11}{2240} = \frac{1}{200}$; and I must here repeat what I have so often before stated to you, that, although experiments have been made which give so low a friction as 4 lb. per ton, that, on an average, M. Navier is nearly right, when we take into consideration the numerous causes of friction. M. Navier considers the power required to draw a given weight "to be independent of the absolute velocity of transit, although there is reason to believe that the tractive power increases with the velocity." Now, it has been said that the friction is the same at all velocities. I cannot fully concur in this opinion. I think the axletree friction may be constant under all velocities; but that, from other causes, there appears to be, I will not call it an increase of friction, but an increase of resistance, the amount of which has not been satisfactorily determined. M. Navier goes on: "We conclude from this, that, in order to transport, with any velocity whatever, constant or variable, a weight, W, to a distance represented by a on a horizontal line, it is necessary to employ the power represented by $\frac{W}{200} \times a$—that is to say, the power necessary to raise the weight to the height $\frac{a}{200}$," or, in other words, to transport a weight any given distance on a horizontal line, is equivalent to raising it the two-hundredth part of that distance in vertical height; and, although this is not quite correct, it is sufficiently so for general purposes. We have before assumed that it is the same thing to go a mile round, as to go over a hill rising twenty feet in a mile. Now, a mile being 1760 yards, or 5280 feet, we have $\frac{W}{200} \times 5280$ as the power required, which is equal to raising the weight 26 feet. But, as the friction varies, I think we have sufficient experience now to say it is about the same thing to rise 30 feet in a mile as to go a mile round; but this is quite independent of the question, whether you should or should not allow on one hand, and deduct on the other, when the slope exceeds

the angle of repose. I have explained to you, on previous occasions, the difference of opinion that exists on this point. Both Mr. Barlow and M. Navier allow the advantage up to a certain point, which they fix at about 1 in 180, beyond which point they consider the whole advantage gained to be destroyed by the necessity of putting on the brake. Now, in practice, we do not find this to be the case, until we come to 1 in 80, or thereabouts; however, we may take, as a general rule, M. Navier's concluding words on this subject:—"The length of the line remaining the same, the amount of power consumed to effect the transit depends entirely upon the length of the line, and the difference of the level of its extreme points." The practical result which I have endeavored to lay before you this evening is, that the cost of transport is the cost of the power combined with the interest of the original cost of the line, and that the calculation of this combined expense must form the element of comparison between different lines of railway.

(To be continued.)

Facts and Observations on Four and Six Wheel Engines.

By JOHN HERAPATH, ESQ.

[Concluded from Page 248.]

London and Brighton Railway.

At Horley station it was intended to have the general engine factory of the company, and a large building has been erected for the purpose. On digging, however, for water, to their surprise, they found it would not do for locomotive purposes. *It literally boils out with the steam, and there is no keeping it in the boiler!* It has been analyzed, but what are its component parts I have not heard. With the ladies, however, who send for it, far and near, for tea, it seems to be a great favorite. The depth of this well is 240 feet, and it is distinguished by an intermittent escape of gas. According to several continuous observations, made by myself and some gentlemen who were there, the bubbling up is like water in rapid ebullition, and occurs very regularly every 70 seconds. This ebullition is preceded for a few seconds by a sort of simmering, which increases rapidly, and ends in the apparent violent ebullition mentioned, and then subsides into a perfect calm. We had no means of collecting and ascertaining what the gas was that escaped. A lighted candle was procured and held down to near the surface of the water by one of the men, but was so badly done as not to be satisfactory. Such as the experiment was, it manifested in the candle no signs of being affected in any way. This water rises to the very top of the tank.

On this line I saw one of the most curious circumstances I have seen upon any line. The Earlswood embankment, which is very lofty, and rests upon a sort of clay base, not long after the opening of the line sank about one-half of it down, from one and a half to two feet below the other half, a field below having risen up to the height of six or seven feet. The down line of rails, therefore, now stands

about one and a half or two feet above the up, and the trains travel, and have traveled for twelve months, at least, over the line in this state, without any alteration of the lines, or any inconvenience to the traffic. The dip is, of course, not long, with a pretty gradual descent and rise; and the trains, by means of the momentum they acquire in the descent, easily mount the ascent.

On another part of the line, farther down, the engineer, finding he had very troublesome materials to deal with, humored them, and allowed a portion of the embankment to remain about ten feet lower than it was intended to be. At a distance, before one comes to it, this dell has a curious appearance; but the trains travel over it daily without the slightest impediment.

One can hardly estimate the uncertain nature of the materials on this line. At one part, scarcely any effort can keep them up to the mark, and at another, where they have been expected to settle, they remain firm and inflexible. The embankment south of the Ouse Viaduct, for instance, one might reasonably have calculated to settle a trifle, however good the material. At its formation, therefore, the embankment was made a little rounder and higher. But what has been the result? Not one bit has it subsided, and, after twelve months' experience, the round back has been taken off, and the embankment reduced to its proper level.

It has generally been supposed that the line to Brighton runs through a monotonous and uninteresting country; but it is not so. The road to Croydon, both before and after the deep cutting at New Cross, particularly after, is distinguished for its beautiful and picturesque scenery. Beyond Croydon, a country of rather a different character opens upon us, but with most lively and interesting features, and in many places very beautiful. Some parts of the line will equal almost any in the kingdom for its rich and pleasing variety. As a whole, it is a very pleasant line to travel over, though occasionally we are immersed in *deep cuttings* and *tunnels*, which one would rather avoid than court.

The total length of line traveled by this company is better than 56½ miles. They have 31 engines to do the work, of which 30 are in an efficient state to take a train. They are all six-wheel engines, except six of Bury's, which are now about to be converted into six-wheel; and all have, except these six and a coupled engine, outside bearings. One of Bury's has already been made into a six-wheel, of which I shall say more hereafter; but the others, since the unfortunate accident last September, have, in deference to the public, never been allowed to go with passenger trains, but are used for goods, or for ballast, engines. They have one coupled goods engine, which they say works well, and is safe at any speed under thirty miles an hour. All their driving wheels have flanches. The detentions of trains from defective machinery, or derangement of engines, have been about a dozen. They have had no broken cranked axles. Two engines have run off the rails, both four-wheel, and, the superintendent of locomotives says, without any assignable cause.

If I mistake not, one of these runnings off was at the time of the

accident; and the other, a running off before entering one of the tunnels, about the same time. If so, I have been informed that the first accident occurred from the badness of the road, or from the rails being covered with *debris;* and I believe from one of the same two causes the second happened. Such, at least, is what I have heard from an engineer who was either present the day of the first accident, or the day after; but, if I recollect rightly, on the very day. It is said both happened on a straight road, a thing which on no other line has happened, nor could happen if the road was clear and good. The play of the wheels of the engines with which the accident happened, was half an inch.

The gauge of this line is the same as that of most of the English lines, 4 feet 8½ inch.

It seems they do not know the gross average load of the trains, but the working pressure is 50 lbs. to the inch. The average consumption of coke is 26 lbs. to the mile. They work expansively, and cut off the steam at two-thirds of the stroke, by which they estimate a saving of 6 lbs. per mile.

No motions are said to be observed in their engines but those common to others, and the most motion is described to be in the four-wheel engine. They have no top-heavy engines, but think the motions, such as they are, irrespective of the condition of the road.

The following table, taken from the returns to the government circular in Sir F. Smith's report, contains the *data* of all the engines they then had.

They are all double cranked axle, and all, except six of Bury's, and a goods engine, have outside bearings.

No. of engines.	Diam. of wheels in feet.			Diam'r of cylinder, inches.	Stroke, inches.	Weight loaded, in tons, &c.			
	Front	Driving	Hind			Front	Driving	Hind	Total
						tons. cwt.	tons. cwt.	tons. cwt.	tons. cwt.
1	3½	5½	3½	13	18	4 7	5 18¾	2 17	13 2¾
1	5	5coupd	coupled	same	same	4 10	5 10¾	2 9½	13 10
2*	3½	5½	3½	14	18	5 0	8 0	2 18	15 18
1	same	same	same	12	18	unknown	unknown	unknown	unknown
13	same	same	same	14	18	5 5	8 9	1 5	14 19
6	4	5½	none	same	same	5 13	8 16	none	14 9
1	3½	5	3½	same	same	4 16	8 17	2 5	15 18

* I suspect one, at least, of these engines is wrong, for the numbers I have direct from Messrs. Rennie are different.

These make only 25 engines, but two more had then (November, 1841,) been ordered of Sharp & Co., three of Fairbairn, and one of Rennie, which I understand are since arrived.

My first trip down the Brighton line was on Saturday, May 21, on No. 28. This engine lurched and slipped much upon the Croydon line, but became steadier on the Brighton, which was in better order. She had no longitudinal motion, but was rough on the platform. She was stated to be five months out, and to consume 29 lbs. of coke per

mile. Her driving wheels were 5½ feet, cylinders 14 inch, and 18 stroke.

There had been a little rain, and I observed the rails to be wet where the ballast was up to them, and dry where the spaces for drainage were. The down road I noticed was best in the Merstham tunnel, and worst in the Clayton. Except a few places, here and there, the road was in very good order. In the Merstham tunnel, I was struck with the very beautiful effect of the two long rows of gas-lights.

Another day I got on the *Merstham*, one of the oldest engines on the line. She has the same dimensions as No. 28, has much play in the axles, and lurches sadly. Her consumption of coke is 25 lbs. per mile.

Each engine, I was informed, ran a fortnight, and was in, three days. They run 122 miles one day, and 102 next.

I was subsequently on No. 27, which they say burns 22 cwt. of coke in 140 miles; No. 26, the *Satellite*, which burns 22 lbs. per mile, by Rennie, a fast and capital engine; No. 22, by Fairbairn, which was the six-wheel engine in the lamentable accident; No. 24; No. 9, an old engine, which lurched a good deal; No. 25, a much tighter engine, steady, and free from wriggle, longitudinal motion, or lurching; No. 29, by Fairbairn, an excellent engine for steadiness.

Monday, June 13, we tried the experiments on the curves already mentioned in my report, with No. 9, one of Sharp, Roberts & Co.'s, and No. 22, one of Fairbairn's, engines. The play of the wheels in No. 9 was 1.1 inch, and in No. 22 it was 0.9 inch.

I had an opportunity one day of getting on the four-wheel engine No. 17, which was at the accident just after the opening. She is now made into a six-wheel. She is 6 feet 1¾ inches from leading to driving axles, and 11 feet 2 inches from leading to trailing. We (the superintendent and myself) had been out to try her steadiness with another, a counterpart of her on four wheels. We ran them both with the same load, about a stage, each at a very rapid rate, but faster with the four-wheel. There was some difference, no doubt, in point of sinuous motion, in favor of the six-wheel, but nothing, we both agreed, that could amount to anything affecting safety. Indeed, had we stood anywhere but on the platform, where the checks of the additional pair of wheels to the vibrations must have been most felt, I doubt if any one but a very experienced hand could have discovered the least difference. From this trial I was satisfied that, as far as security goes, the additional pair of wheels is unnecessary.

The greater part of the engines of this company appear to be Sharp, Roberts & Co.'s. They are good, strong engines, but, in my opinion, there is too much rigidity and dead weight about them. They have also too great play in the axles, which causes them to lurch very much. Fairbairn's engines are longer and more flexible in the horn plates, and run infinitely smoother and steadier, though, except the lurching, which in some of them is excessive, Sharp, Roberts & Co.'s work very pleasantly.

The best engine, however, that I was on, was the *Satellite*, one of

Rennie's. The workmanship and working of this engine are each excellent, and she is well adapted for taking a good load at a high speed. She cuts off the steam at two-thirds of the stroke. Her length of boiler is 8 feet, diameter of it 3 feet 4 inches; fire-box, length 2 feet 11½ inches, breadth 3 feet 6¼ inches, height 3 feet 6 inches; smoke box, 2 feet 3 inches long, 4 feet 4 inches broad, 5 feet ½ inch high. She has 99 tubes of 2 inches diameter, 4 of 1.625 inch, and their length is 8 feet 5 inches. Her heating surfaces are 52 feet in the fire-box, and 450 feet tubular; area of steam passages, 15.12 inches, of exhaust pipe 24.06, and of blast pipe 8.29. Weight 16 tons—that is, 5 on leading, 8 on driving, and 3 on trailing, wheels; cylinders 15 inch, stroke 18; driving wheels 5½ feet, other wheels 3 feet 6 inches. To what pressure the valve is screwed I was not able to ascertain, in consequence of the curious manner in which the scale was graduated.

I am informed this engine has taken, without assistance, from London to Brighton, 15 loaded carriages, at 35 miles an hour. When I traveled with her, she was blowing off steam the whole way. Two circumstances contribute to her steadiness—the suppression of the back pressure from the size of the ports and blast pipe, and the placing of the suspending springs below the frame, by which the centre of gravity is lowered fifteen inches. She contains several minor improvements in the details, and is altogether a very excellent engine.

The engine-men have, as on the Birmingham and Gloucester, rewards for every pound of coke they save per mile, upon a certain amount, according to the engine. This excellent plan causes the men to be exceedingly careful, and saves a great deal to the company.

I have only to add that I have received every facility, in the prosecution of my labors, from the directors, secretary, and engineer, and, in general, from the officers and servants of the company. They seemed desirous that every information I could desire should be afforded me, and nothing withheld, or kept back. § Railway Mag.

On the Causes of Injury to Steam Boilers. By C. W. Williams, Esq.

In my last paper on this subject, I explained some of the causes of those injuries to which steam boilers are exposed, and dwelt on the circumstance that the sediment assumes two distinct forms, namely, that of a solid crystalized incrustation, and of a loose mud-like body, held merely in suspension. I showed that the first could not be the cause of injury to the iron plates of the boiler, inasmuch as it was itself a good conductor of heat; whereas the second—the floating matter—would become a positive non-conductor, if allowed to subside, when the boiler had been at rest for some hours, and when it would assume the dry hard consistence of plaster of Paris.

I now propose to give further proofs of the conductibility of this solid crystalized incrustation, and draw some important inferences therefrom. I had two pins constructed, to act as conductors, each three inches long, and three-quarters of an inch square, one made of

iron, and the other cut from a large slab of incrustation taken from the interior of a marine boiler. These were inserted into separate vessels, containing water, the one end projecting half an inch into the water, through the side, and the remaining part projecting outwards, to receive the heat from a powerful gas-burner. These vessels were so protected, that no heat could reach them, except what passed longitudinally, and exclusively through the conductor pins; consequently, the water received no heat except what was conveyed, by conduction, through those pins.

By means of the *iron* conductor pin, the water was made to boil in 13 minutes, and, by the *incrustation* conductor pin, in 17½ minutes. That the pins themselves were not raised to any inconveniently high temperature was proved by the fact, that, when suddenly removed from the flame, which was very intense, and while the water was fiercely boiling, the pins themselves were at a temperature so low as to allow the finger to be pressed against them without inconvenience; it certainly did not appear to be above 500 or 600 degrees—a temperature far too low to produce any injurious effect on their structure. This experiment resembled the well known one of taking a kettle, containing boiling water, from the fire, and placing it on the hand, for an instant, and without injury. I may here observe, that I was not able to discover any difference between the temperature of the two conductor pins.

Now, since no heat was received by the water, in either case, except what passed longitudinally through the conductor pins, it is manifest that the entire heat which raised the water to the boiling point, and maintained it in a state of active ebullition, must have passed through a vertical section of the side of the vessel, of but three-quarters of an inch square. This experiment, therefore, proved, first, that this three-quarter inch surface of the boiler plate was sufficient for the transmission of a quantity of heat out of all proportion greater than could have been transmitted by such area under ordinary circumstances; second, that this incrustation (which was crystalized sulphate of lime) possessed an admirable conducting property; and, third, that no possible injury could be sustained by the conductor itself, so long as its temperature remained so low.

The first of these facts shows how erroneous have been our previous modes of estimating the evaporative power of any kind of boiler, or fuel, by calculations drawn from the mere areas of the exposed plates; while it proves that much may yet be done in this department of the boiler. The second shows that, in this crystalized state of the deposit, it cannot be the cause of injury to the plates, although the uncrystalized, or loose, matter, if allowed to settle and become hard, becomes a mischievous non-conductor, and the direct source of injury from overheating and bulging. The third proves that, if the recipient body to which the heat is conveyed be able to absorb the heat as fast as it is passed through the conducting body, no injury can be sustained by the latter, seeing that this solid mass of incrustation, (hitherto supposed to be a bad conductor,) itself remaining unaffected, was equal to the conveyance of a very powerful heat,

through no less than three inches; while, in fact, it never reaches to above half an inch in thickness on those parts of boiler plates which are exposed to the greatest heat.

Now to apply these facts, and the inferences to which they lead, to practice. We find that, so long as the water is maintained in contact with the plates through which heat is conveyed by conduction, no injury will be sustained. But the question arises—what is there to interrupt this contact, and what other recipients than water are to be found in boilers? In land engine boilers, no injury can arise to the plates from any circumstance connected with the furnace, or fuel, beyond the ordinary wear and tear, (the sources of which will be hereafter examined,) if due attention be paid to cleanliness in the interior, and maintaining the water at its proper level. Marine boilers, however, from their peculiarity of construction, are subject to another source of injury, and which is too often destructive of the plates connected with their furnaces, and the parts adjacent. This peculiarity consists of numerous vertical narrow passages. In these, the steam, as fast as it is generated, becomes, by reason of its ascending current, so mixed with the water, as seriously to obstruct the free and continued access of the latter to the plates. This also takes place to the greatest extent in those parts which are exposed to the highest temperature, since such ascending current of steam is necessarily the greatest where the heat is greatest, namely, in the side plates of the furnaces. The consequence is, that these side plates, through which the heat is conveyed to the interior of such narrow passages, are more frequently overheated and bulged than other parts, though exposed to even a still higher temperature from the direct action of the flame.

The heating of the side plates of the furnaces of marine boilers may, therefore, be said to arise solely from the circumstance, that, by reason of the conflicting currents of steam and water in those narrow passages, or water-ways, the recipient, being then a mixture of water and steam, (too often of the latter alone,) the heat cannot be taken up as rapidly as the metal conveys it, and the usual consequences of overheating necessarily follow.

This interposition of steam, where water alone should be found, and its inevitably injurious consequences, were strikingly illustrated in the first boilers of the transatlantic steam-ship, the *Liverpool.* In these boilers, the water spaces were above 5 feet perpendicular, and but 5 inches wide, thus leaving a space of but 2½ inches for the water approaching the side plates of each of the furnaces, and the steam generated by the heat received through such plates. This steam was necessarily so great in quantity as to prevent the access of the water, and, in fact, became itself the recipient of the heat from the furnaces; the consequence was, that the plates became overheated, bulged, and cracked, and extensive injury was sustained by them during every voyage. Not unfrequently they required to be wholly removed and replaced, at a considerable expense, before a new voyage could be commenced.

That steam, in fact, was the recipient of the heat in those narrow passages, where water should always predominate, was proved by a

very simple and conclusive experiment of the engineer, during one of his voyages. He introduced a trial pipe in the space (erroneously, in this instance, called the *water*-space,) between two of the furnaces, and on a level with the fuel—the inner end opening into such space, and the outer end projecting outside the boiler, and being furnished with a stop-cock. The result proved his anticipation; for on trying this pipe, when the furnace was active, he could never draw off any thing but *steam*. This circumstance was conclusive, that although the water continued at its proper level in the boiler, yet, by reason of the confined nature of the passages, and the absence of a free circulation and access of the water, (at the very place, which, of all others, required its continual presence,) the steam, a bad recipient, had usurped its place. This source of injury continuing, the furnace side plates, as constantly were deranged, while the roofs and other parts remained sound to the last.

From the instance here adduced, it does not follow that any given width of water-space is necessary, or that narrow spaces must always be injurious. I have frequently observed that spaces of but three inches wide between the furnaces have been unattended with injury to the plates. The cause of injury, then, arising from the predominance of steam instead of water, is rather to be traced to other circumstances connected with the circulation of the water in the boiler, and the aids or impediments it receives from the peculiar construction or arrangement of the flues.

The main practical consideration, then, in seeking to protect the plates of boilers from overheating, is, that it is not to the fire, or furnace, that attention should be directed, but simply and solely to the nature of the recipient to which the heat is conveyed, for in this will be found to rest the whole question of injury. This will be objected to by those who have hitherto anticipated danger from hard firing and incrustation, and the want of due proportions between the fire and flue surface. Yet I state the position broadly, after the fullest investigation and the most conclusive proof, that, if we look to the recipient and its heat-absorbing properties, and attend to the interior of the boiler, and preserve all right in these respects, we shall do all that is practicable towards preventing injury from overheating, or what is erroneously termed "burning the plates."

Let us now inquire what are the several recipients of heat which present themselves in ordinary boilers. These are—

1. Water.
2. Steam.
3. Air.
4. Deposit crystalized.
5. Deposit uncrystalized.

The two latter have already been examined. I have now to speak of the three first mentioned, and this I will do in my next communication.

Liverpool, Feb. 7, 1842.

Lond. Mech. Mag.

Concrete, its Introduction, Composition, Uses, and Comparative Expense.

Concrete was first used in this country by Sir Robert Smirke, at the erection of the penitentiary at Millbank, afterwards at the under-setting of the walls of the new custom house, and has been generally used by the abovenamed architect in the public buildings since erected under his care, especially at the club house of the Oxford and Cambridge University in Pall Mall, where the whole area of the building, and to the extent of two feet beyond the line of the lowest footing, was covered to a depth of 2½ feet, the depth being increased to four feet under all the walls that rise to the roof; in the specification of the last named building it is thus described. "For the grouted stratum, clean river gravel is to be provided, and mixed with lime ground or pounded to a fine powder; it is to be well mixed with the gravel, twice turned over before it is wheeled to the excavation, and it is to be thrown from a height of not less than six feet in every part. A man to be kept treading down and puddling the mass as it is thrown down; the proportion of materials to be six parts of gravel, to one of Dorking, Merstham, or Haling, stone lime." It has now become, in the present day, the most favorable expedient resorted to for artificial foundations. Mr. Ranger, of Brighton, improved the above hint by using hot water to facilitate the setting, for which he took out a patent for making artificial stone. A detailed account of the application of Mr. Ranger's artificial stone to the building of docks and river walls at Chatham and Woolwich, is given in the first volume of the Journal, being a paper by Lieut. Denison, from the Papers of the Corps of Royal Engineers. Analogous to concrete is beton, from which it differs in broken stone being used, instead of gravel, in the proportion of two of stone to one of lime, or pozzolana, of Italy; a description of which, taken from the Franklin Journal, appeared in vol. 3, page 265, of your valuable periodical. Since the introduction of concrete, some little difference of opinion as to the proportions of materials, and manner of mixing them, has arisen among engineers. I therefore give the composition from several specifications:—No. 1. The concrete to consist of five parts of clean gravel, perfectly freed from loam, or clay, with a proper proportion of small gravel and sand, as well as large, and one part of lime measured dry; the lime to be mixed into a perfectly smooth, uniform paste, as for the mortar, but with more water, and then thoroughly mixed with the gravel.—No. 2. The concrete to be composed of sandy gravel and well burnt lime, in the proportion of three of the former to one of the latter. The gravel to be free from all earthy matter, and the pebbles not to exceed one inch in diameter. The lime is to be used in a hot state when slaked, and to be immediately mixed, using no more water than is sufficient to incorporate them. After being twice turned, it is to be wheeled on to a stage ten feet high, and let fall into the trench; it is not to be puddled, or disturbed in any way, until perfectly set.—No. 3. All concrete must be composed of gravel, perfectly clean, and

mixed with fresh, well burnt lime, in the proportion of six of gravel to one of lime. The lime and gravel to be mixed in a dry state, and a sufficient quantity of water afterwards added.—No. 4. Concrete to be composed of good lime, gravel and sand, in the proportion of one-seventh to one-ninth of lime, and it should be laid in about twelve inch layers, or courses, and pitched from a height of ten to twelve feet, neither should it be disturbed until properly concreted and set.

In the above five opinions, including that of Sir Robert Smirke, we have the relative proportions of gravel and lime, varying from three to nine; and No. 1 states the lime and water to be first mixed, in which No. 2 nearly coincides, whilst No. 3 insists on the gravel and lime being first mixed, and then the water added; Nos. 4 and 2 coincide that the concrete is not to be disturbed after it is thrown into the trench, whilst Sir Robert Smirke expressly says that parties are to be employed puddling the mass. The whole are agreed in specifying that the material is to be thrown from a height. From considerable practice and experience in the mixing of concrete, I think that the lime need not be ground, but simply mixed with the gravel, and then, by the addition of water, it will fall to an impalpable powder; also, that it is unnecessary to be at the expense of puddling the mass after being deposited in the trenches, neither is there any advantage to be derived from discharging the mixture from a height, both of which operations increase the expense of the concrete; and as the concrete in the act of setting expands in bulk, I think that alone a sufficient proof of the inutility of both of the abovementioned operations, their tendency being to condense the mass, whilst its own natural tendency is to expand. With respect to the proportion of lime and gravel, I think the less lime the better will be the concrete, and that the proportion of 8 to 1 of lime is decidedly better than 3 of gravel to 1 of lime. As to the quality of materials employed, the lime must be stone lime, fresh from the kiln; that from chalk will not do, and hydraulic, or lias, lime is to be preferred to stone limes. With respect to gravel, if obtained from a pit, the ochreous, or ferruginous, is to be preferred; and if loam is present, so as to soil the hand, the gravel must be washed; if the gravel be obtained from rivers by dredging, alluvial and vegetable deposits are to be avoided; and if the gravel contain vegetable refuse, it must be screened, or washed. Shelly, sharp gravel, is the best; the proportion of small or large pebbles, and the due quantity of sand, is soon learned with a little practice.

As to the uses of concrete, it is principally adopted as an artificial foundation, and from four to six feet is a sufficient depth, and extending two feet beyond the space to be occupied with the building. The following testimony of the utility of concrete is from Weale's Bridges, page 31: "Piling will probably never be found more safe than a body of concrete; the latter cannot be too much esteemed for its durable and almost imperishable nature, besides being quite as safe, and, perhaps, more durable, than piling;" and from the paper of Lieutenant Denison, before alluded to, we have the following ratification of its uses: "Concrete cannot be advantageously employed as a building material." "It may be employed with advantage in backing retain-

ing walls." I. K. Brunel, Esq., C. E., has used concrete as a foundation, nearly exclusively and universally, in the bridges on the Great Western Railway; and, in the celebrated bridge of Maidenhead, the land arches are backed with concrete to the depth of ten and a half feet, and the abutments of the large arches are also backed with concrete. In culverts underneath embankments, the same able engineer has extensively used concrete as a backing material, the brickwork being kept thin, and then enveloped in a mass of concrete, in the form of a polygon, of six sides, or of the form of two truncated cones, with their bases joined.

Concrete was used on the Great Western Railway, wherever it could be employed, as a backing material; its use is now rapidly extending to the provinces, and bids fair to supersede all other means now employed for making a foundation; it is much improved by being mixed with oxide of iron, smiths' scales, and roasted iron-stone, or any material containing iron. As regards the comparative expense, brickwork, being the most common building material, has been taken as the standard of comparison with concrete for price; and its cost, in most districts, will be found from one-third to one-sixth the price of brickwork; taking a cubic yard as the quantity of each material, the latter will cost 5*s.*, and the former 21*s.*, both, to a great extent, being regulated by the vicinity of brickyards, and the facility of obtaining gravel. I have known concrete executed at 3*s.* 3*d.*, 3*s.* 6*d.*, 4*s.*, 4*s.* 6*d.*, 5*s.*, 7*s.* 6*d.*, 8*s.* 6*d.*, and 11*s.* 6*d.* per cubic yard, although the most common price is 7*s.* 6*d.*; as to brickwork, the general price is 21*s.*, and the range is from 14*s.* to 27*s.* 6*d.* per cubic yard; the London price being 25*s.* per cent. dearer than the country. The facility of obtaining lime regulates the cost of concrete; the price of lime per cubic yard, measured dry in clots, at Dorking in Surrey, is 11*s.*; Barrow in Leicestershire, 21*s.*; Bulwell in Nottinghamshire, 9*s.* 6*d.*; Breadon in Derbyshire, 15*s.* 6*d.*; Harefield in Buckinghamshire, 16*s.* 6*d.*; Fulwell, Durham county, 9*s.* The measures of lime, also, vary much; in some places it is sold by the cubic yard, measured dry, which is decidedly the best method adopted; it would be desirable if it was universal. It used to be sold in London, by the hundred, as it was called, not of weight, but a measure, a yard square, and a yard and inch deep, which will be equal to sixteen or eighteen bushels, but it is now sold by the cubic yard. The Fulwell and Barrow lime is sold by the quarter, eight of which make a ton and a half. Lime is also sold by the boll and chaldron; a chaldron will be about three and a half tons, a single horse cart about six bolls. In agricultural districts, the bushel, boll and quarter, are used; in colliery districts, the chaldron and ton are the standard of measure. With respect to the cost of gravel, provided it can be obtained on ground belonging to the company, the getting, screening and cartage, will cost 1*s.* 6*d.* to 2*s.* per cubic yard; if it be obtained from the gravel pits of the country, the charge will be, per ton, from 2*s.* 6*d.* to 2*s.* 9*d.*; if screened, 3*s.* 3*d.* to 3*s.* 10*d.*; if broken, 6*s.* 10*d.* A cubic yard will weigh from 24 to 27 cwt. If the gravel is dredged, or brought from the shores of a river, the cost will be 2*s.* 6*d.* per yard, or nearly the same as from the

pit. The prices of the various operations of getting, screening, and washing gravel, are respectively 10*d.* and 12*d.* per cubic yard. The price of excavation is also included in the price of concrete in all railway specifications, which will be about 4*d.* per cubic yard, as, generally, the excavation is of limited extent, and, consequently, more expensive than an extensive excavation; and when the gravel is obtained on the ground of the company, or proprietor, the excavation is a double operation, the hole having to be refilled with other materials in lieu of the gravel obtained. From the experience of several thousands of yards and variety of situations, I find the cost of mixing the materials, or, as it is termed, concreting, to be 1*s.* per cubic yard, and, taking the proportion of material at 5 to 1, the following will be a fair estimate of the cost of concrete :

	s.	*d.*
1 cubic yard of lime,	12	6
5 do. gravel, at 2*s.* 6*d.*	12	6
Labor, mixing at 1*s.* per yard,	6	0
6 yards of excavation, at 4*d.*	2	0
Waste, contingencies and profit, at 1*s.*	6	0
6 cubic yards, at 6*s.* 6*d.* =	39	0

Concrete will set in twenty-four hours; the specific gravity is 125, or about the same as brickwork, although brickwork is sometimes 165 lb. per cubic foot. Lieut. Denison gives the strength of concrete $S = \frac{l W}{4 b d^2}$. The constant S being 9.5, and comparing concrete to York paving, the proportion is as 1 to 13.

Civ. Eng. and Arch. Jour.

An Investigation of the Comparative Loss by Friction in Beam and Direct Action Steam Engines. By Mr. W. Pole.

This paper, consisting almost entirely of mathematical investigation, and involving the application of the differential and integral calculus, was read in abstract. Its object was, to show the futility of an objection frequently urged against the "direct action," or "Gorgon," engines, from their alleged increased friction. The results of this investigation appeared to be, that

The vibrating, or oscillating, cylinder engine has	a *gain* of 1.1 per ct.
The direct action engine, with a slide,	a *loss* of 1.8 "
Ditto, with a roller,	a *gain* of 0.8 "
The "Gorgon" engine,	a *gain* of 1.3 "

showing that the direct action engines, as generally constructed, and as adopted by the government for the steam vessels in the navy, has rather the advantage over the ordinary beam, or side lever, engines. In the conversation which ensued, it was agreed that the allowance which had been usually made for friction in steam engines had been

over-rated; that, in reality, the friction rarely amounted to more than 1 lb. per square inch; and that, owing to the perfection to which the construction of machinery had now arrived, a further gain might be anticipated. Although the law of "friction being independent of the area of the rubbing surface," as given by Poisson and others, was impugned by some of the members, it was allowed, that as both kinds of engines had in the paper been treated analytically by the same rule, the results for both would, in an equal ratio, approach towards truth, and that, therefore, the conclusions arrived at might be received as correct.

The author further explained the nature and objects of his paper, which had not been fully understood, and illustrated the mode of analytical reasoning by which he had arrived at his conclusions. He then proceeded to answer the objections which had been raised against the laws of friction adopted by him, and to comment upon the mode of experimenting of Vince and others; showing, on the other hand, by quotations from the recorded experiments of Amonton, Coulomb, Rennie, and Morin, and from the works of Gregory, Brewster, Emerson, Playfair, Barlow, Farey, De Pambour, Poisson, Pratt, Whewell, and Mosely, that the views he had taken were correct. He also noticed the variations produced by attrition, and by the introduction of unctuous substances between the rubbing surfaces. These views were corroborated by several members present, some of whom had been quoted as authorities, and the propositions involved appeared to be generally received.—*Trans. Inst. Civ. Eng.*

Lond. Athenæum.

Machinery for Excavating, or Cutting, and Removing Earth.

Many have been the attempts to supersede, by means of machinery, the use of hand-labor in the tedious and laborious operations of cutting and removing earth, for leveling inequalities of the surface, forming canals and docks, and clearing the beds of rivers. These mechanical contrivances have necessarily partaken of the same general features, viz., moving peckers and shovels, or scoops, constructed and arranged in various ways, and actuated by wheels and levers, in a variety of forms and combinations, from the simple and well known dredging apparatus, commonly worked in our harbors and rivers, to the elaborate and gigantic new American excavator, which, under the absurd cognomen of the "Yankee Geologist," has been proclaimed to the world as capable of removing mountains.

Without intending, in the slightest degree, to detract from the merits of this American invention, which we hear from disinterested parties, who have witnessed its performance, to be one of paramount importance and vast capability, we think it necessary, in order to qualify the extravagant statements given in some of the periodicals of the day, both foreign and English, respecting its astonishing powers, to state what are the leading points on which its claims to novelty are founded.

In order to show this more clearly, it will be desirable to mention,

in a brief way, the objects and features of the several machines for excavating and removing earth, which have been the subjects of patents within the last twenty years. The first of these we find to be the invention of George Vaughan Palmer, of Worcester,—a machine to cut and excavate earth, granted 8th June, 1830. This machine is mounted upon wheels, intended to advance upon a temporary railway, laid upon the surface where the excavation is to be made, beneath which a hole is dug to commence the operations in. There are a number of peckers in front of the machine, which, by vibratory action, dig into, and thereby break up, the earth. A consecutive series of buckets, connected by an endless chain, are brought down into the disturbed and broken ground, and scrape up the soil, stones, &c., which are carried away up an inclined plane, in the manner of the ordinary dredging apparatus. The machinery is worked by a winch and toothed gear, and advances upon its railway as the earth is broken and removed.—See Vol. VII, page 314, of our Second Series.

Sir Thomas Cochrane, Knt., obtained a patent, 20th October, 1830, for apparatus to facilitate excavations, sinking and mining; but this is a pneumatic contrivance, merely to prevent the percolation of water into a tunnel, whilst in progress of formation. See Vol. VII, page 304, Conjoined Series.

Mr. G. V. Palmer, of Worcester, had a second patent granted, 24th January, 1832, for improvements in machinery or apparatus for excavating, and which he called an excavating and self-loading cart. This contrivance much resembled an ordinary cart upon two wheels, drawn by horses. Under the cart were placed the cutting, or excavating, instruments, formed something like the share and breast of a plough, which excavators were capable of being lowered, so as to take into the ground and break up the soil to any required depth, as it advanced; or they might be drawn up out of operation, in order to allow of the cart traveling on ordinary roads, when proceeding to, or returning from, its work. The running wheels of the cart were broad, and their felloes hollow, and in these hollows were transverse partitions, formed by plates, which constituted the bucket wheels. On the cart advancing, the ploughs, or cutters, penetrated into and broke up the ground, and turned the soil sideways into the buckets of the running wheels, which, as they revolved, raised the soil, and, in turning over, let it fall on to inclined edges, by which it was conducted into the cart.—See Vol. I, page 278, Conjoined Series.

In December, 1833, a patent was granted to Mr. Thomas Affleck, of Dumfries, for his invention of improvements in the means and machinery for deepening and excavating the beds of rivers, removing sand-banks, bars, and other obstructions to navigation. This, however, consisted merely of apparatus, which, when agitated by the rolling waves, or rise and fall of rivers, disturbed and broke up the mud, sand, or gravel, for the purpose of enabling it to be washed away by strong currents, or freshes.—See Vol. IV, page 273, Conjoined Series.

An apparatus to facilitate and improve the excavation of ground, and the formation of embankments, invented by Mr. William Brun-

ton, engineer, of London, was made the subject of a patent, dated 2d November, 1838. A part of this invention was a series of hook-shaped cutters, fixed in a frame, one in advance of another, and which, being connected to machinery, were forcibly projected into the ground, and made to plough it up in grooves; each cutter, as it advanced, cutting and preparing the way for the next cutter in succession. The other parts of the invention applied to the arrangement of stages, and the order in which a series of workmen were to dig and remove the soil. Also, the manner of depositing soil for the formation of embankments; compressing it to give solidity; and conducting the earth-wagons, upon tram-ways, by endless ropes.—See Vol. XVII, page 284, Conjoined Series.

Mons. L. J. A. Ramel, a foreigner, obtained a patent in England, dated 19th March, 1838, for his invention of improvements in machinery for excavating and embanking earth, for the construction of railways, and other works. The specification of this patent does not set out in very clear terms what are the features of novelty proposed, but speaks of the "system of a lever." As far as we can understand the subject, it seems to be merely the adaptation of a long lever as a crane, which works vertically, to raise loads of earth in a box, in place of employing hand-barrows, passing up inclined planes, or of pitching the earth from stage to stage by hand-labor. This lever is mounted upon a platform, with running wheels, for the convenience of passing it from place to place, upon a railway; and the lever, to one end of which the loaded box is attached, is worked by a cord, or chain, connected to the other end, and to a winding drum, or barrel, and windlass; and when the load of soil is conducted to the place of deposit, it is let fall into a cart, by opening the bottom of the box.

An invention of certain improved machinery for cutting and removing earth, was communicated to Mr. William Newton, of Chancery Lane, by a foreigner, for the purpose of obtaining a patent, which was granted on the 27th March, 1839. This invention is a peculiar arrangement and construction of apparatus, mounted in a carriage upon a temporary railway, in which a series of rotary cutters, or peckers, working in inclined positions, are made to break the ground below, at an angle of about forty-five degrees, as the carriage proceeds; and also to throw the earth, thus broken, into a consecutive series of buckets, attached to an endless chain, which, by traveling vertically, takes up the broken earth to the top of the excavation, and delivers it into a series of troughs above, which troughs, by moving in a transverse direction, carry away the earth and deposit it in carts, or otherwise, as convenience may require.—See Vol. XVI, page 57, Conjoined Series.

Mr. W. Scamp, of Woolwich, obtained a patent, dated 16th February, 1841, for an application of machinery to steam vessels, for the removal of sand, mud, soil, and other matters, from the sea, rivers, docks, harbors, and other bodies of water. This invention consists merely of a barrel, studded all over with spikes, which, being mounted upon an axle, was suspended by lever arms from the vessel, and, on being lowered down to the bed, or bottom, of the river, the barrel

was made to revolve, as the vessel advanced, by a traveling endless chain, extending from a pulley, or spur wheel, on the axle of the propelling wheels; or, by other rotary means, to a pulley on the axle of the barrel, so as to cause the mud, sand, and other materials on the bottom, to be disturbed, or broken up, by the spikes, and, on mixing with the water, to be carried away by the current.

These are all the schemes which have been proposed and brought before the public, under the protection of letters patent in England, within the last twenty years, until the introduction of the American invention above alluded to.

This machine, which we are not permitted at present to lay before our readers in all its details, consists of a horizontal platform, mounted upon wheels, carrying a strong jib-crane, and also a steam engine. From the end of this jib-crane, the excavating tool, or cutter, is suspended by chains and pullies, which allow of its swinging in a forward direction; and the back part of the tool, or cutter, is attached to a rod, or beam, sliding on rollers, which, being acted upon by chains and toothed wheels, in communication with the steam engine, causes the cutter to be projected, with great force, against the earth required to be broken up.

The mechanism and the suspending chains, connected with the steam engine, and with the projecting rod, or beam, affords the means of regulating and determining the course in which the cutting tool shall move forward; and by means of a small hand-lever, a workman, standing upon the platform, is enabled to direct the advancing cutter through the ground, in a horizontal line, or through any inclined, or curved, course, up to a perpendicular; the movements of the pendulous chains determining the course of the cutter, whilst the sliding beam projects it forward.

The excavating tool is formed as a scoop, with strong tangs, or teeth, in front, to break the earth as it enters, and a sharp cutting edge to take up the broken fragments.

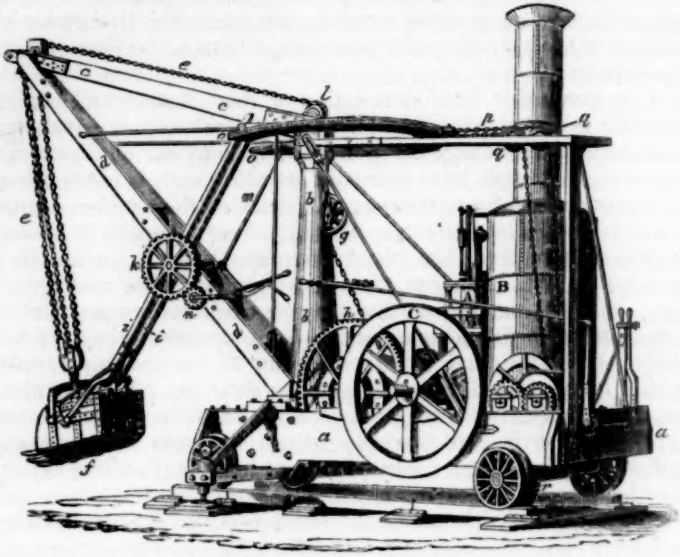
The machine having been moved upon its railway to the place where it is required to excavate, the platform is then made fast, pro tem., in that situation, and the steam power of the engine brought to act upon the mechanism, by sliding clutches, or other contrivances. The pendant tool, or excavator, is then forced forward by chains, connected to the projecting beam, and passed round a rotary drum, driven by gear from the engine; and, at the same time, the pendant chain is drawn up, or let out, as may be necessary, to allow the excavator to advance in the required course. When the projecting beam has carried the excavating tool forward to its extent of action, in a horizontal cut, the suspending chain, from the crane-jib, will raise the loaded scoop, (or the projecting and raising of the scoop may be simultaneous, as the workman shall direct,) which loaded scoop, when brought to its highest position, may be conducted to one side of the excavation by the swinging jib, and the contents let fall into a cart, by opening the back of the scoop; all which operations are effected through the agency and power of the steam engine, under the direction and regulating hand of the workman.

It is only necessary further to say, that, by turning the jib of the crane to the right or left, the cutting of the earth may be performed at any angle to the direction of the machine, and, consequently, to a very considerable extent,—viz., a circuit of forty or fifty feet,—without shifting its situation; but when a change of place becomes necessary, the fastenings by which the platform was secured must be withdrawn, and the power of the steam applied to move the whole, upon its turning wheels, to the next place where it may be required to be made stationary.

Having given this brief description of the construction and mode of working the new American excavating machine, we conclude our present report by stating the points of novelty which it may fairly claim over others that have preceded it. Firstly, it is locomotive; its movements, and all its operative parts, deriving their powers from the steam engine which it carries. Secondly, that the earth is broken up and carried away from the place excavated by one instrument, (the scoop,) acting with immense effect, through the power and agency of steam. Thirdly, that the cutting may be made with equal facility at any inclination to the horizon, and to a great extent around the spot on which it is stationed, by the direction of the workman, without requiring to be moved from its place. Fourthly, that, by this machine, a channel may be cut through a hill, with the proper slopes for its sides, and a level base correctly formed, the excavated earth being simultaneously removed. Fifthly, the capability of cutting many feet below the base on which the machine runs, by lengthening its chains and guide-beam; which last feature renders it also applicable to working under water, when placed in a vessel, for removing sand-banks, bars, and beds of mud.

The engine and boiler, by which the various parts of the machine are put into operation, are shown at A B.—*a a* is the framework, provided with wheels, by means of which the whole apparatus is capable of being moved along a temporary railway, as the machine digs away and removes the earth before it. The crane-post is shown at *b b*, at the upper end of which is placed the crane-jib *c c*, supported by the diagonal beam *d d*, which is also used for carrying certain wheel-work and apparatus for effecting the required movements of the shovel. At each end of the crane are mounted pullies, over which a chain, *e e*, passes from the shovel, or excavator, *f*, and from thence down the centre of the crane-post, and under the carrier-pulley, *g*, to a windlass, or capstan, on the axis of which is mounted a large toothed wheel *h*, taking into a pinion upon the main driving-shaft, on which is mounted the fly-wheel *c*. The shovel, or excavator, is connected by swing-joints to the forked end of diagonal arms *i i*, which are furnished with chains, attached to each end thereof. These chains pass once round pullies, mounted upon the axle of the toothed wheel *k*; and hence, on rotary motion being communicated to the said axle, the diagonal arms *i i*, and consequently the shovel *f*, will be caused to move upwards or downwards. The end of the shovel is connected by hinges to the other parts thereof, and retained in its proper position, during the operation of digging, by means of a bolt, or pin, which

may be withdrawn, by means of suitable apparatus, when the filled shovel is raised by the chain *e e*, and swung round to the required position; the shovel will then tilt over, depositing the excavated earth in a wagon, or other required receptacle.



Upon the axle of the guide-pulley, on the top of the crane-post, is a beveled toothed wheel *l*, taking into a similar wheel, mounted upon a diagonal shaft *m*, at the lower end of which is a beveled pinion, taking into another, mounted upon the axle of a pinion *n*, which latter pinion is capable, by means of hand-levers, of being shifted in and out of gear with the wheel *k*; by which arrangement, the chain *e*, passing over the guide-pulley, and communicating rotary motion thereto, will cause the pinion *l*, and shaft *m*, to revolve, and thereby, through the intervention of the pinion *n*, and wheel *k*, effect the required motion of the diagonal arms *i i*, and shovel *f*, the attendant being able to arrest the motion thereof, at any time, by means of the hand-levers connected to the pinion *n*.

The horizontal motion, or swinging round, of the crane is effected by means of the horse-shoe shaped pulley *o o*, affixed to the crane by cross-rods; to this pulley each end of a chain, *p p*, is fastened, which chain, having passed round the periphery thereof, is conducted downwards, by means of guide-pullies, *q q*, passing once around an axle, driven by wheel-work, connected to the engine, which wheel-work is capable of being shifted in and out of gear with the main shaft, by the attendant, through the intervention of a hand-lever; by this arrangement, the chain, *p p*, is put into motion at discretion, thereby causing the horse-shoe shovel, *o o*, to revolve, and, with it, the crane

and shovel, or excavator. The machine is propelled along its temporary railway, as the work progresses, by means of a toothed wheel *r*, affixed on the axle of one pair of running-wheels, and connected to the motion of the engine by suitable gearing.

When the operation of excavating commences, the shovel is caused (by the loosening of the chain *e e*) to assume a nearly perpendicular position, the teeth thereof being turned towards the earth; motion then being communicated to the several parts, by means of their respective trains of wheel-work, the chain *e e* is gradually drawn tight, and wound around the capstan, or windlass; during which operation the arms *i i* are brought into action, forcing the shovel into the ground by the means before described; hence it will be perceived that the shovel, or excavator, is operated upon by power exerted in two directions, the one through the medium of the arms *i i*, causing it to be thrust into the earth, the other through the medium of the chain *e e*, and its appendages, causing it to be lifted therefrom; by which combined action, and suitable speeds of driving gear, the shovel will describe a curve in ascending, the commencement thereof being just in front of the machine, and the end thereof vertically under the front of the crane-jib. The shovel being filled with earth, and raised to this point, is swung round, by means of the horse-shoe shaped pulley *o*; and the bolt, which secures the ends thereof, being withdrawn, the contents will fall into the wagon, or other required receptacle; after which, the crane is again swung round, and the various parts put out of gear, when the shovel will descend, in order to operate upon the earth as before.

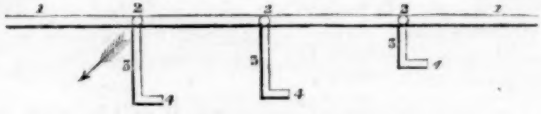
This peculiar arrangement of apparatus, it will be seen, is applicable only to operations performed on land; but a machine on the same principle, suitably modified for the intended work, has been constructed for the purpose of dredging harbors, deepening rivers, or other such operations, a description of which, with a more minute account of the first machine, we shall lay before our readers at a future time.

Lond. Jour. Arts & Sci.

Notice of the great Explosion at Dover. By CAPTAIN STUART, *7th Royal Fusiliers.*

An operation in engineering was successfully performed near Dover to-day, which, from its magnitude and novelty, must be a subject of deep interest to every person acquainted, in the least degree, with practical science. It was the removal of an enormous mass of the cliff, facing the sea, which formed an obstruction to the line of railroad. To give you a distinct idea of its position, it may be necessary to inform you, that a portion of the cliff which was penetrated by the tunnel made through Shakspeare's Cliff, gave way about two years ago. About fifty yards of the tunnel were carried away, and a clear space was so formed for the line of railroad, with the exception of a projecting point, which, prior to the slip alluded to, was the extremity of the part of the cliff pierced by the tunnel, and to remove which was the object of the operation in question. Mr. Cubitt is the engineer

under whose management it took place. The expense of clearing it away, by the tedious process of manual labor, would have exceeded £12,000; and this consideration, as well as the time that would have been lost, induced him to try the bold experiment of blowing it away with gunpowder. It cannot be denied that there was apparent danger in the undertaking, for the weight of the mass to be removed was estimated at 2,000,000 tons, and the quantity of powder used was more than eight tons, or 18,000 lbs. 12,000 lbs. was the quantity used in blowing up the fortifications of Bhurtpore; and this, I believe, was the greatest explosion that ever (previously) took place for any single specific object. I had several opportunities of seeing the preparations for this grand event. The front of the projection was about one hundred yards wide; this front was pierced with a tunnel about six feet in height, and three in breadth; three shafts, equidistant from each other and from the entrances to the tunnel, were sunk to the depth of seventeen feet; and galleries were run, one from each shaft, parallel with each other, and at right angles with the line of the tunnel. These galleries varied in length, the longest having been 26 feet, the shortest 12 feet; and, at their extremities, chambers were excavated in a parallel direction with the tunnel. The following rude sketch may give a clearer idea of it.



1. The Tunnel. 2. The Shafts. 3. The Galleries. 4. The Chambers.

In the chambers, the powder was deposited in three nearly equal quantities; it was done up in 50 lb. bags, and the proportion in each chamber was contained in a wooden case, nearly as large as the chamber itself. Ignition was communicated by means of a voltaic battery. Conductors, 1000 feet in length, were passed over the cliff, one to each chamber, and the electric fluid was communicated in a shed, built for the purpose, on the top of the cliff, about fifty yards from the edge. The explosion was conducted by Lieut. Hutchinson, R. E., who, you may recollect, was engaged, under General Paisley, in blowing up the wreck of the Royal George. Two o'clock, P. M., of this day, the tide being then at its lowest ebb, was fixed on for the explosion to take place. The arrangements were the best that could be made to preserve order, and, as far as possible, prevent danger. A space was kept clear by a cordon of the artillery, and the following programme was issued.

Signals, January 26, 1843.

1st, Fifteen minutes before firing, all the signal flags will be hoisted.
2d, Five minutes before firing, one gun will be fired, and all the flags will be hauled down.
3d, One minute before firing, two guns will be fired, and all the flags

(except that on the point which is to be blasted) will be hoisted again.

These signals were given exactly at the specified time, and, when the expected moment arrived, a deep subterranean sound was heard, a violent commotion was seen at the base of the cliff, and the whole mass slid majestically down, forming an immense debris at the bottom. The success of the undertaking equaled the most sanguine hopes, and exceeded the expectations, of all. It was a splendid triumph of skill, and reflects the highest credit on Mr. Hutchinson and Mr. Cubitt.

Dover, Jan. 26, 1843. Edinb. New Philos. Jour.

On the Introduction into Scotland of Granite, for Ornamental Purposes, by Messrs. Macdonald and Leslie, of Aberdeen. By Professor TRAILL, F.R.S.E., M.W.S., &c. &c.

The first idea of employing the refractory, but enduring, material, granite, in sculpture, appears to be due to the ancient Egyptians. Those who have enjoyed opportunities of examining their colossal buildings, have acknowledged the precision, and even delicacy, of the figures and ornaments with which that ingenious people contrived to enrich their architecture. Specimens of their sculpture in granite, which have for three thousand years resisted the action of the elements, and the yet more destructive influence of barbarous invaders, still astonish us by the high polish of their surfaces, and the delicate finish of their details. Even a visit to the Egyptian Saloon of the British Museum will prove that, in accuracy of muscular delineation, and in the communication of absolute *fleshiness* to the lips and features of some of the figures there preserved, the ancient Egyptians evinced a high perfection in the art of sculpture, in a material of the most imperishable kind, on which few succeeding artists have ventured to employ the chisel.

In our own times, the fabrication of slabs, pedestals, and vases, in hard porphyries, and in granite, has been carried to great perfection in Sweden. The quarries of Blyberg, at Elfdalen, for many years, have furnished materials for Swedish ingenuity and skill. The elegant forms and high finish of their works in those refractory materials, have contributed greatly to the splendor of the Swedish capital, and are known and admired over Europe. Yet, though our own mountains yield no less beautiful and durable materials, it is surprising how long we have remained without any attempt to apply them to the purposes of ornamental art. It is true that, for more than half a century, Aberdeen has exhibited a city chiefly built of hewn granite; that, more lately, this same material has been employed in the construction of Waterloo Bridge, in London, and in a few other works; and that Cornish granite appears in the pedestals of a few statues in some of our towns. But the idea of giving a polish, equal to that of

ancient Egypt, to our granite in works of considerable size—of introducing this splendid material as a domestic ornament in our halls and saloons, and as lasting memorials of departed worth in our cemeteries—is undoubtedly due to two citizens of Aberdeen, Messrs. Macdonald and Leslie, who carry on extensive works in that town, where the gray granite of Aberdeen, and the rich red granite of Peterhead, are cut into an endless variety of ornamental articles, which receive the highest polish.

A late visit to their establishment convinced me that these gentlemen have reduced to practice the difficult problem of giving any required form to so stubborn a material as granite, and of communicating to its surface an exquisite polish, which show it to be well suited for domestic ornament, and as a superb decoration for the abodes of rank and opulence. The rich warm tint of the Peterhead granite, in particular, will harmonize better with the gilded ornaments and gorgeous hangings of a modern gallery, or superb saloon, either as tables or as pedestals for works of art, than furniture made of the most costly woods, or even than the snowy marble of Carrara.

For monumental work, this enduring material possesses advantages over the best marble. In our climate, the effects of rain, sudden frosts, and succeeding thaws, are soon perceptible on Carrara marble, or any other kind, exposed freely to the weather. Marble thus soon loses its glossy surface; it contracts greenish stains from the vegetation of minute *Byssi;* and inscriptions, in a few years, from these causes, become illegible. The polished granite of Aberdeenshire retains its polish most perfectly under all atmospheric changes, does not contract any stain from vegetation, and, unless wantonly mutilated, will transmit the inscription engraven on it to distant ages. The sharpness of the Egyptian hieroglyphics, carved in a very similar rock, three thousand years ago, at this day, proves the durability of granite carving. A beautiful cenotaph of red granite, from the works of Messrs. Macdonald and Leslie, has been exposed to all the vicissitudes of our changeable climate, for six or seven years, in the churchyard of Falkirk, and appears in the full lustre of its original polish, as if it were erected yesterday.

Fine specimens of granite monuments, by the same artists, may be seen in the noble new cemetery at Glasgow, which are chaste in design, beautiful in execution, and seem calculated to bid defiance to every destroying influence, except wilful injury.

On visiting the establishment of Messrs. Macdonald and Leslie, at Aberdeen, I saw several finished specimens, and many works of this material in progress, as I was conducted through the different departments, by the intelligent and most respectable head of this interesting and new employment of national art and industry.

The gray granite is of a close grain, and contains more mica than the red. It is brought from quarries on the Dee, a short way above Aberdeen. The red granite is of a larger grain, abounding in felspar and in quartz, intermingled with small specks of mica, and bears a strong resemblance to the syenitic rock of which the finest ancient Egyptian monuments are fabricated. This comes from the vicinity

of Peterhead, and is brought by sea to the works. Both are susceptible of a fine polish, which they retain unimpaired by the weather. Blocks of almost any size may be obtained free of flaws, or imperfections. In the sawing room, several blocks were then under the machines, which are moved by a 14-horse power steam engine. I observed one block, ten feet long, cutting into six or eight slabs. The saws are, as usual in such works, of soft iron plates, secured in a frame, and operate on the stone by means of quartz-sand and water, applied as in slicing marble. No emery is requisite in these operations, the particles of siliceous sand being sufficient to cut the quartz, the hardest material in the granite. Frequently, fourteen saws are used in a single frame, and occasionally they have had as many as eighteen employed at once on a single block of stone. The progress of the work, of course, is slow; it requiring a whole day to cut a groove, two-thirds of an inch in depth, in the granite. The slabs, when cut, are polished by moving one over the other, by appropriate machinery; siliceous sand being first interposed, and then emery, of various degrees of fineness, until the requisite degree of lustre is obtained.

The first dressing of the granite blocks into parallelopipeds, cylindrical masses, or other curved forms, is performed by *hand-picks*, with short handles, and heads about four pounds in weight, which the workmen, from long habit, wield with surprising accuracy. The surfaces are then reduced to a regular form, by means of well-tempered chisels, urged by iron mallets; the chisels require a very particular temper, which must be neither very hard nor very soft, else they would either lose their edge by *chipping*, or fail to cut the stone. I observed that they frequently require sharpening in the more delicate kinds of work. The chisel is held by the workman very obliquely to the surface of the stone, and he separates very small particles at a time.

I have already described the polishing of plane surfaces. Circular forms, such as *stelæ*, frusta of columns, as pedestals for busts, vases, and the like, are fixed in well-contrived lathes, and are whirled round by machinery, while the sand and emery are applied to their surfaces by means of thick plates, or bars, of iron, previously forged to their various curvatures, when they are not cylindrical.

I saw a large vase, about four feet in diameter, prepared by the chisel for the process of polishing. Its graceful curves were beautifully and accurately cut by the chisel; the iron bars, 1 or 1¼ inch in thickness, neatly forged to its various curves, lay beside it, ready to be applied when it was fixed in the lathe.

In the warerooms were many finished articles of great beauty and elegance, such as well-executed pedestals for busts, or vases, of red and gray granite; chimney pieces of the same material, numerous slabs, tables, and seats for halls, and beautiful vases, in a considerable variety of forms, rivaling those of classic Italy in shape, mural tablets for monuments, and some altar-formed tombs of magnificent size. These last were made to order. Some of the chimney pieces are in-

tended for the Earl of Lauderdale's residence, Thirlstane Castle, and some of the slabs for Sir Robert Peel, &c. &c.

I was surprised at the neatness of the *lettering* on all the monuments, and saw the men at work. The monument is first finished in other respects; the letters are carefully traced with a dark, or light, crayon, according to the color of the stone, and the workman traces the outline of the letter on the stone by light strokes of a fine-edged chisel, held nearly vertically; deepens the lines by a succession of similar blows, while the chisel is held very obliquely, removing the stone in the state of powder, so as to avoid chipping. Roman capitals are thus easily formed; but I saw old English, or German, letters, with a superfluity of curved lines, carved on the granite with equal precision.

But the most remarkable work which I saw in this establishment, was the neatly finished statue of the late Duke of Gordon, intended to be erected in one of the streets of Aberdeen. It is 11 feet high, of a single block of granite. This statue was modeled by Mr. Thomas Campbell, the sculptor, and has been transferred from the model to the granite by Messrs. Macdonald and Leslie. Two men were at work on the drapery, at the period of my visit. They worked with fine chisels, held very obliquely, and urged on by iron mallets of two or three pounds in weight. The attitude of this statue is simple, and the features are said to be very like the original. This, which may be considered as the first specimen of a British statue of a single block of granite, in emulation of the durable monuments of ancient Egypt, is a memorial by the county to the late noble and gallant officer, and, when erected, will be a distinguished ornament to Aberdeen.

Another great public work, executed by the same artists, is already erected in that town. In 1842, the splendid public markets of Aberdeen, excelled by none in Europe in elegance, were opened. The great saloon, containing the fruit and vegetable market, a magnificent hall, 300 feet in length by 100 feet in breadth, has within it a noble fountain of highly polished Peterhead granite. An octagonal basin, constructed of polished blocks, stands about one-third the length of the hall from the southern extremity. From the centre of this basin rises a shaft, ten feet high, supporting two circular cups, or shallow vases, one placed over the other. The lowermost is formed out of a single block, seven feet three inches in diameter, and the upper has about half that width. A constant jet of water rises from the centre of the upper cup, flows over its edges into the lower vase, which also overflows, in a thin sheet of limpid water, into the basin below, whence water is drawn for all the purposes of the market. I have seen no fountain in Britain so fine as this. It resembles in form, and surpasses in material, the finest fountains I saw in Spain; yet it was erected by Messrs. Macdonald and Leslie for £200.

The same artists are at this moment engaged in executing a similar monument for Lord Prudhoe, which, I understand, will cost about £200.

Indeed, considering the difficulty of working so hard a material, I

was surprised at the moderation of their prices, for articles produced at their interesting establishment.

For instance:—

1. A hall-table slab of polished granite, measuring 4 feet long by 21½ inches wide, costs £4 15s.

It may be stated, that slabs may be furnished, of any required size, for from 12s. to 14s. for each square foot of surface.

2. Pedestals for busts, square or columnar, with plinth, and an ovolo when columnar, of the usual size, for £10.

2. Mural monumental tablets, with vase, trusses, &c., from £6 to £9, according to the size.

4. Mural tablets, with base, cornice, and pedimented top, from £10 to £12.

Lettering, of the usual size, is charged 4s. 6d. per dozen of letters.

5. An elegant *Tuzza formed* vase, of classic shape, 4 feet 9 inches in diameter, and standing 2 feet 9 inches high, on a beautiful pedestal, costs £40.

6. They have also executed columns of granite, for halls and vestibules, at prices equally reasonable, in proportion to the size and style of decoration. But of all the purposes to which they have hitherto applied the granite, it seems especially suitable for monuments of every kind, both from the beauty of the highly polished material, and its imperishable nature under all vicissitudes of the weather.

The extent and perfection to which these gentlemen have carried the working of this very refractory, but beautiful, stone, may be considered as forming an era in British art, and require only to be more generally known, to be appreciated and encouraged by public taste and munificence.

Edinburgh, March 18, 1843. Ibid.

Roof over a Panorama.

The first paper read was a "Description of the Roof suspended over the Panorama in the Champs Elysées, Paris," by M. Hittorff. The Hon. Secretary, Mr. Bailey, observed, that although the Germans attribute the invention of panoramas to Prof. Breisig, of Dantzic, it is generally admitted that they are of English origin, and that the first was exhibited in 1793, by Robert Barker, in the city of Edinburgh. The most important building for such exhibitions, one far surpassing any at that time existing in foreign countries, was erected in London by Mr. T. Horner, and is known as the Colosseum, Regent's Park. The plan is a polygon of sixteen compartments, whose interier diameter measures about 123½ feet. The dome is constructed of timber, curved and arranged upon the principle of Philibert de Lorme, and is covered with copper. In the centre of the building are two concentrical cylinders, supporting three galleries, as well as supporting the centre of the dome, or roof. The rotunda, since erected in Paris, surpasses in magnitude even this vast edifice. Among the various designs for embellishing the Champs Elysées, M. Langbois suggested

the idea of a rotunda for the exhibition of panoramas. The ground was granted to him for a term of forty years by the Municipal Council, on the following conditions:

1. The diameter of rotunda to be 130 feet.
2. The roof to be conical, and without a central kingpost.
3. The rotunda to be lighted by a cycle of glazed sashes.
4. The intervention of any obstruction between the sashes and the wall of the rotunda (thereby casting a shadow upon the canvas) to be carefully avoided.
5. And, finally, all these data to be severally complied with at the least possible expenditure.

Considering the difficulty of constructing a building of such dimensions without a central kingpost, and the great expense of arched timberwork, together with the solid structure of the walls, indispensably requisite to resist the thrust of the wooden vault, the artist resolved to apply (in the construction of the new building) the principle of suspension adopted for bridges, by means of chain cables. The site of the rotunda not allowing of the adoption of stays, fastened at a distance from the building, it was necessary so to contrive the buttresses that they should hold the cables and resist their tension. Their number, amounting to twelve, gives a subdivision of the wall of the rotunda into arcs; and it may be considered, at the level of the stone cornice, as a polygon, whose sides adjacent to one and the same buttresses, offered a two-fold force opposed to the strain of the cables. In this way the resistance was obtained nearly at the expense of the cornice and the wall; and by adopting two circles, with the cables passing between these circles, the upholding chains can be strained as required. These cables pass at an angle upwards, over vertical rods, which rest on pivots on the inner edge of the cornice wall, which is about 3¼ feet in thickness; they then rest on the outer edge of the cornice wall, and the ends of the cables are carried to the abutment walls, and are there fastened. The building was commenced in October, 1838, and covered in January, 1839. It attains a mean height of nearly 50 feet, occupying a surface of nearly 21,653 square feet, and its circumference is composed of a mass of building above 16 feet in depth, having three stories distributed into apartments over a space of 520 feet; and the cost of the whole was about £13,000.—*Inst. of Brit. Arch.*

Lond. Athenæum.

Bridge over the River Wear.

The next paper was by Mr. D. Bremner; it contained a description of the stupendous bridge over the river Wear, on the line of the Durham Junction Railway, which connects the city of Durham with Newcastle, South Shields, and Sunderland, and is now destined to form a portion of the great chain of railway towards Edinburgh. It is built on the spot originally selected by Mr. Telford for a bridge on the line of the projected great road to the north; it was designed by

Messrs. Walker and Burges, after the model of Trajan's bridge at Alcantara, and, with some modifications to suit the locality, has been constructed, under Mr. Harrison, the engineer of the railway, by Messrs. Gibb, of Aberdeen, whose perseverance and skill in the execution of the structure, and in contending with the difficulties of it, are highly to be praised. The bridge consists of four nearly semicircular arches, of 160 feet, 144 feet, and two of 100 feet, span, respectively, with three arches of 20 feet span each, at either end forming abutments; the total length is 810 feet, by 21 feet wide, and from the top of the parapet to the top of the foundation, at the point of the greatest depth, is 156 feet 6 inches. It is entirely constructed of freestone from the Pensher quarries adjoining; and as a plain, simple structure, containing boldness of design, with excellence of execution and economy, rivals any other work of the kind in Great Britain. The means employed by the contractors for executing the work, appear to have been very complete. The north arch, of 100 feet span, containing about 980 tons of stone, was entirely turned in twenty-eight hours by two of the cranes employed in laying the stones, giving an average weight of 17½ tons of stone laid by each crane per hour. The bridge was commenced in 1836, and finished in 1838, occupying 714 working days, and cost about 35,000*l.*—*Inst. Civ. En.*

Mining Journal.

Experiments on Locomotives.

Experiments on the Grand Junction Line, with six-wheel Engines.

Name of engine.		Gross load. Tons.	Mean speed in miles per hour.	Coke consumed. Per mile in lbs.	Coke consumed. Per ton per mile.
Phalaris,	May 30, 31, June 1, July 5	59.2	23.05	37.03	.62
Prometheus,	June 5, " 6, " 7, " 8	56.7	22.53	34.3	.60
Prometheus,	June 11, " 12, " 13	52.8	22.30	41.9	.79
Phalaris,	June 14, " 15, " 16	62.6	22.08	38.5	.61

Experiments on the London and Birmingham Line, with Bury's four-wheel Engines.

Description of engines.	Gross load in tons.	Mean rate of speed in miles per hour.	Coke per ton per mile.
12-inch cylinders and 5-feet wheels,	50.15	30.51	.47
	70.95	28.53	.4
	81.61	21.85	.35
12-inch cylinders and 5-feet wheels,	50.77	31.29	.55
	69.76	29.82	.41
	83.53	19.42	.29

Lond. Mech. Mag.

Engineering Science.

The tunnel on the line of the Sheffield and Manchester Railway will be three miles in length, upwards of 600 feet below the surface, or summit, of the hill at its greatest height, and in rock formation throughout its entire length. The works were projected and commenced upwards of two years ago, under the direction of Charles Vignoles, Esq. Five shafts were opened, at about half a mile distant from each other, for the purpose of proving the formation, of facilitating the driving of the drift-ways, and, ultimately, of ventilating the tunnel. Whilst these were in progress, the drift-ways were carried on from each side, or face, of the mountain; the distance, or length, driven, on the eastern side, extending to nearly 1000 yards, and from the next shaft 180 yards. The junction between these two portions of the drift-way was effected on the 17th of September, and the levels, when checked, on a tie-bench, at the point of meeting, had varied but nine decimals, or one inch nearly, and the range was within less than two inches of being geometrically true. When it is considered that this has been attained whilst driving upwards of half a mile through hard rock formation, it must be admitted to be highly creditable to the parties engaged in directing it.—*Dub. Ev. Post.*

Lond. Athenæum.

Practical & Theoretical Mechanics & Chemistry.

On the Properties of the Crank.

Till very lately, the contrivance of a mechanism which would generate power, was thought quite a legitimate problem. Government fostered this belief by a proclamation which it has not yet recalled, offering a tempting reward for its solution. The error has finally passed into the hands of a few wrong-headed individuals, ignorant alike of the history and of the principles of mechanics; but we have only passed from one extreme to the opposite. We rarely meet with candidates for the proffered reward which is so temptingly held out to the discoverer of the perpetual motion; but we are still infested by

another fallacy, which, notwithstanding its opposite tendency, belongs to the same category. We are no longer told that mechanism *per se* can generate power; but the opposite opinion, that machines, independently of the friction of their rubbing parts, are capable of destroying power, has still numerous and intelligent adherents. Thus we are perpetually informed of the great and needless waste of power which results from obliquity of action in the moving parts of a machine, and are not unfrequently called upon to examine cumbrous and expensive contrivances for rendering these actions direct, and for recovering—it may be more than recovering—the power believed by the inventor to be unnecessarily thrown away.

From this to the conception of a perpetual motion, is only a step; for admitting a machine to be capable, independently of friction, of destroying power, we might rationally expect that the inverse action of the same machine would be capable of generating it. Both doctrines are equally inconsistent with the fundamental laws of mechanics, and with that law of mechanism which informs us that, in every combination of the ordinary mechanical powers, supposed to be divested of friction, the efficient power is of precisely the same value at all points, understanding the efficient power to be the force multiplied into the velocity.

To develope this principle more fully, it is necessary to refer to the law of *virtual velocities*. This law informs us that, if two weights balance each other when suspended from unequal arms of a lever, that they are to each other inversely as the lengths of these arms; and if the lever be made to vibrate on its fulcrum, the distances through which the weights move are directly as the lengths of the arms from which they are suspended; so that, if each weight be multiplied into the distance through which it moves, the two products are equal to each other. Thus the descent of 1 lb. through 10 inches would, for example, be accompanied by an ascent of 10 lbs. through 1 inch; so that whatever is gained or lost in intensity of pressure, is lost or gained in distance. The same law holds true of combinations of pulleys, and also of the inclined plane and bent lever, although its application to these cases is not so obvious. It is also true of any combination of these mechanical elements; for if, in any machine, we induce motion, it may be found that the force transmitted, combined with its velocity, is equal to the force applied, combined with the distance through which it has advanced. This is true, however ill-contrived, and however ill-constructed, the machine may be; it delivers over the whole, and exactly the whole, amount of force which put it in motion. Part of this force it expends in overcoming the friction of the rubbing surfaces, including the resistance of the air, and gives up the rest as effective power to accomplish the particular purpose for which the machine is intended.

If, then, this principle be correct, it follows that, in every machine composed of the ordinary mechanical powers, the quantity of power developed, that is, the dynamical effect of the moving force, is the same at all points, and that the power is transmitted without any other change in its value than is caused by the loss resulting from

friction. In a mechanical point of view, the inventor can profitably direct his attention only to two objects—economy in the material and labor necessary to the first construction of the machine, and the diminution, as far as practicable, of the effects of friction. Were all friction avoided, it would be matter of absolute indifference by what means the required changes of motion might be produced; it would then be of no moment whether we employed the reciprocating, or the rotary, steam engine—whether we adopted short, or long, connecting-rods—whether we used the crank, or the sun and planet wheels, further than the mere expense of workmanship is concerned. Beyond this, and the comparative amounts of friction, we have absolutely no criterion for estimating the superiority of one mode of construction over another.

It would not be difficult to establish the principle involved in these broad statements by general reasoning applicable to every possible combination of machinery; but we shall, in the mean time, confine our attention to the condition of the steam engine crank—the groundwork, at present, of much pernicious fallacy and needless controversy. This may seem inconsistent with the simplicity of the crank, for no machine can possibly be more elementary in its character, and, we might suppose, could be more easily understood, and give rise to fewer doubts respecting the nature of its action. So simple is it, that we can hardly reckon it an addition to the axle of which it forms a part; it is merely a *crook* upon it, and appears to have been the earliest contrivance for the purpose of converting a revolving into a rectilinear motion, and the reverse. It is figured in the old machines of the Egyptians, the Chinese, the Greeks, and the Romans; and it has been employed to move the pistons of the cylinders of water-pumps, since the time of Aliotti, in precisely the same way as it now moves in the steam engine. A radical misconception has, however, arisen regarding it, and a multiplicity of crude and abortive contrivances have been proposed to supersede its use—all less elementary in their character, and every way inferior in point of practical application.

To show the grounds upon which this misconception rests, it is observed that there are only two points in a revolution of the crank at which the connecting-rod (*supposed here to be infinitely long*) forms with it a right angle, and it is only at these points of full-leverage that the whole force of the steam transmitted through the rod is acting to produce effective motion of the crank. These positions are denoted by figs. 1 and 2. But, again, in the positions indicated by figs. 3 and 4, the connecting-rod and crank are in the same straight line—technically called the "position on the centre," or passing the "line of centres," and the "dead-power points"—in which the leverage of the crank is nothing, and, consequently, no power, however great, acting through the rod, could produce rotary motion in it, in either direction. The force at these two positions is exerted upon the crank-centre alone, whilst, at the points of greatest leverage, no part of it is thus exerted—the whole there tends to cause the crank to turn upon its centre. At all intermediate positions, the force transmitted through the rod is resolved into two effects—one acting to give the crank rev-

olution, and another acting upon its centre; and it is further clear, that, the leverage of the crank increasing from zero to a maximum during the first quarter revolution, and diminishing from a maximum to zero during the next quarter, the efficiency of any force transferred to it through the connecting-rod will, in like manner, increase from zero to a maximum, and diminish from a maximum to zero.

Fig. 1.

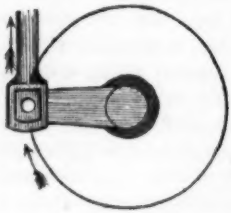


Fig. 2.

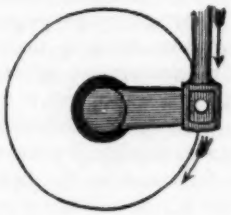


Fig. 3.

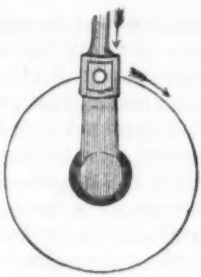


Fig. 4.

Were we, therefore, to stop short in our examination at this point, we might also conclude that the crank is a "losing lever," and set about equalizing this *apparently* most unequal action, and saving the power so prodigally thrown away in our steam engines. Another step in the analysis serves, however, to convince us that the pressure produced upon the crank-centre does not imply a loss of dynamical force; that when the apparent loss is greatest, there is no loss whatever; and that, at every point, the effect produced is in direct relation to the quantity of steam producing it.

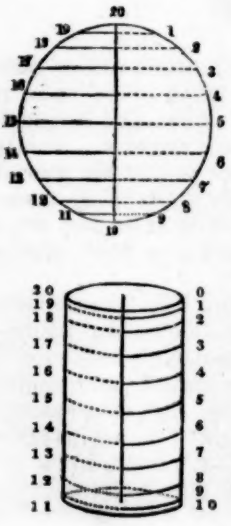
To show how these conclusions are arrived at, let it be recollected that, at the two extremes of the line of centres—the neutral points—the greatest apparent loss takes place. A moment's reflection is, however, sufficient to convince us that, at these instants, there cannot possibly be any loss; there is no power expended, and, consequently, none wasted. The supply of steam, the element of power, is closed—the communication with the boiler is cut off—the steam in the cylinder has done its work, and only waits to be dismissed the instant

the eductive passage is thrown open for egress. Communication being opened with the boiler, the entering steam finds the piston almost in contact with the end of the cylinder at which it enters—it insinuates itself into the vacant disk—the piston yields to the pressure, and begins to move towards the opposite end of the cylinder. At first its progress is slow, but gradually accelerates, till, on reaching the middle of the cylinder, it moves with the full velocity of the crank in its circle. But, from the moment that it passes that point, that is, half way to the end of its course, the velocity of the piston begins to be retarded, and its final stoppage prepared for. At last, the rectilineal motion, having dwindled to nothing by insensible shades, altogether ceases. The expenditure of steam, mean time, corresponds to the motion of the piston—first, it expands, with a continually increasing movement, to the point of half-stroke, and then its expansion begins to diminish, till, finally, it ceases to produce any effect, and is released from its confinement by the opening of the eductive port.

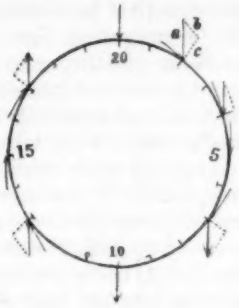
Such is the history of the progress of the piston, from one end of its cylinder to the other. During the first half of its course, its motion is gradually accelerated by increasing increments; it then begins to be retarded, and is finally brought to rest by decrements of motion in the inverse order.

Our next business is to trace the simultaneous motion of the point which, by its connexion with the piston, is carried round the circumference of a circle, while its mover progresses in its reciprocal strokes in the cylinder, and to inquire how this gradual change from rest to motion, and from motion to rest, can be rendered consistent with the uniform motion of the crank in its circle. To facilitate this part of the inquiry, and show the relation which these motions bear to each other, it is necessary to have recourse to a simple diagram, such as that annexed, in which the circle represents the path of the crank, and the figure below it, the steam cylinder of corresponding length. The numerals on both figures represent the places of the crank and piston at given instants of time. The motion of the crank is supposed to be uniform, and, therefore, its orbit is supposed to be divided into twenty equal parts, the first ten numerals being placed on the descending, and the other ten on the ascending, side of the circle. The length of the stroke of the piston—which is equal to the diameter of the circle—is similarly divided into ten unequal parts, showing the places of the piston at those instants of its stroke which correspond with the contemporaneous points of the crank's orbit—the first ten numerals corresponding both in the circle and cylinder with corresponding points of reciprocation and revolution.

Fig. 5.



Now, in order to arrive at a proper estimate of the relation which exists between the pressure of the steam on the piston, and the quantity of effect produced in revolving the crank, and that part of the pressure of the steam which produces no motion, and which is therefore said to be lost,—let us construct such a diagram as that annexed of the crank's orbit; and let the arrows placed vertically denote the force of the steam transferred through the connecting-rod—those placed as tangents, the part of that force tending to produce revolution in the crank—and the lines directed to the centre, the apparent loss. Then, knowing the amount of steam pressure transferred, we can easily ascertain the relations of the lines *a*, *b*, and *c*, to one another, and, thereby, the force acting in the direction of the circle, and also that acting upon the centre of the crank at those points. Other figures, showing the relations of the forces, may, in like manner, be constructed at any of the other given points of the circle—the conditions of the construction being, simply, that *a* represent, in magnitude and direction, the force transferred through the connecting-rod; that *b* be parallel to the direction of the circumference (or tangent) of the circle at that point, and that *c* be directed from the same point to the centre; and further, that *b* and *c* be produced till they meet, and thus determine each other's magnitude in relation to the constant quantity, *a*. Constructed upon a sufficiently large scale, the relation of the forces may be ascertained with sufficient accuracy by the compasses; and only a very slight knowledge of calculation is necessary to verify the results contained in the following table, which is calculated for all the points marked in fig. 5, upon the assumption that the force of the steam $a = 100$ lbs.



Points in fig. 5.	Area moved over by the crank.	Force in the direction of revolution: Value of *b*.	Pressure upon the centre: Value of *c*.	Relative velocity of crank to piston.
20	0°	0.00	100.00	Infinite.
1 and 19	18°	30.90	69.10	3.236
2 " 18	36°	58.78	41.22	1.701
3 " 17	54°	80.90	19.10	1.236
4 " 16	72°	95.11	4.89	1.051
5 " 15	90°	100.00	0.00	1.000
6 " 14	108°	95.11	4.89	1.051
7 " 13	126°	80.90	19.10	1.236
8 " 12	144°	58.78	41.22	1.701
9 " 11	162°	30.90	69.10	3.236
10	180°	Mean 63.138	Mean 36.862	Infinite.

According to this table, then, only about 63 per cent. of the steam

transferred through the connecting-rod to the crank tends to produce revolution of the crank round its centre, while 37 per cent. of the force tends to produce motion in the direction of the centre. Now, it is this 37 per cent. (something more than one-third of the whole force) which is considered to be lost by the crank; and the reasoning which has led to that conclusion is in every way similar to that here adopted. But, without proceeding further, it might be suggested, that, as the centre of the crank is fixed, and prevented from yielding, except to the force acting to produce revolution, that no *power* can possibly be expended in this pressure; for no motion is produced towards the centre, but only in the direction of the circumference. That which we call *power* is not pressure, but pressure combined with motion. The ram of the pile-driving engine may be suspended by the shears for any length of time, without expenditure of force—it becomes no lighter by suspension, and not a whit less ready to fall, in obedience to the force of gravity, when it is released. Yet, if mere pressure be taken as power, that power ought constantly to become less and less by continuance of action; and not only the ram, but every body which presses upon the earth's surface, ought, according to this doctrine, to be giving out their *power;* and, applying the supposition to the case in hand, there must be an enormous waste of *power* constantly incurred in the steam engine, by the pressure of the steam upon the interior of the boiler in which it is generated.

It is to such absurdities as these that we are led by confounding pressure and power. We must, therefore, bear in mind, that, in all calculations respecting power, we must attend to the space passed over, as well as the force exerted in that space; and that a force of 1 lb., moving through ten inches, is equivalent in effect, as already stated, to a force of 10 lbs. moving through a space of one inch; that is, in calculating the quantity of effective power, a greater velocity is equivalent to a greater force. Applying this to the case in hand, let the connecting-rod and crank be so nearly in a line, that a pressure of 100 lbs. upon the piston exerts only a pressure of 1 lb. in the direction of rotation; then does it follow, that, if the piston advance minutely in the cylinder, the extremity of the crank will advance one hundred times as far along its path. The quantity of effective power developed is thus equal to the power expended; for the quantity of motion generated in the machine is precisely what is due to a pressure of 1 lb. acting through one hundred times the advance of the piston—or, what is the same thing, to the pressure of 100 lbs. acting through that advance itself. This is true of any other minute motion of the piston, and may be verified as nearly as the approximate results contained in the table given above will allow, for any part of the crank's revolution. Thus, by reference to the table, it will be observed, that, when the crank has advanced 18° in its orbit, the force of the connecting-rod, tending to produce revolution of the crank, is 30.9 lbs., and that the relative velocity of the crank is to that of the piston as 3.236 to 1. Now, supposing these relations to remain the same while the piston advances through one inch, then, the pressure being 100 lbs., the effective power of the piston is expressed by 100 lbs. × 1 =

100 lbs.; and the pressure in the direction of the crank's orbit being 30.9 lbs., and the velocity 3.236 inches, the effective power is expressed by 30.9 lbs. × 3.236 = 99.99 lbs.; that is, 100 lbs. very nearly, the deficiency arising from the fractions of the factors 30.9 and 3.236 not being complete.

What is thus true of a minute portion of the stroke, holds equally true of it as a whole; for while the piston moves through the length of the cylinder, which is equal to the diameter of the crank circle, it moves the crank through one-half of a revolution. Now, the length of the diameter of a circle bears to the length of its semi-circumference the following relation—

Diameter : semi-circumference : : 2 : 3.14159,

that is, very nearly the ratio 63 : 100. In other words, if the length of the stroke be 63 inches, the space passed over by the crank in its orbit will be 100 inches. Now, the mean force on the crank, in the direction of revolution, is shown by the preceding table to be also 63 to 100, which shows that the mean force in the piston is greater than the mean force in the crank, in precisely the same ratio in which its velocity is less than the velocity of the crank. And, therefore, the effective power in the one is equal to the effective power in the other. To render this, if possible, more clear—the pressures on the piston and crank being inversely as the spaces through which they move, and the motive force in the cylinder being 100 lbs., moved through a space of 63 inches, (the length of the stroke,) we have, as the effective power of the piston, 100 lbs. × 63 = 6300 lbs.; and, for the motive power given out in the crank, 63 lbs., moved through a half revolution of 100 inches, that is, 63 lbs. × 100 = 6300 lbs.

The conclusion, then, that the power of steam is by no means disadvantageously applied through the crank in the ordinary way, rests upon these facts:—1. The velocity of the crank in its circle is in the inverse ratio of the pressure upon it. 2. The mean pressure on the crank during the whole revolution is less than the pressure on the piston, in exactly the same proportion that the space moved over by the latter is less than the space described by the former, so that the whole effect is equal to the whole power. 3. The steam is not at all expended at the neutral points, and its expenditure at every other point is exactly proportioned to the pressure it gives out in useful effect. 4. The velocity of the piston is the ratio of the force acting at each instant on the crank to produce revolution. Making, therefore, allowance for friction, we may rest satisfied that we receive through the crank, in actual work done, all the power of the steam applied to it in the cylinder—and that no force whatever is lost by obliquity in action.

This is, indeed, proved in the most satisfactory manner by the practical fact, that the crank engines of Cornwall are in every respect as effective, and do as much work, as the average of those which have no crank. And it may be worthy of remark, that, as far back as 1837, it was proved in the most careful way, by Mr. Smith, of Manchester, that the work done by the crank engines of Charleston and

Wheal Kitty, constructed by Mr. Sims, was within ten per cent. of the power employed—a fact which, of itself, would seem enough to induce contrivers of crank substitutes to reconsider the question.

As a means of converting the reciprocation of the piston of a steam engine into continuous revolving movement, the crank possesses some singular and beautiful properties, which distinguish it from every other mode of producing that conversion, and which appear to be so perfectly adapted to the nature of steam, and the constitution of solid matter, that we are indebted to it materially, although indirectly, for the very great advantages we derive from the modern steam engine as a source of mechanical power. Ingenuity has been taxed to the utmost to find substitutes for it, which should remedy its imaginary defects; but, after many vain efforts, it is found that the crank is the magic rod through which the mighty force of the element can be transmitted peaceful and docile. It may be said, that successful substitutes have been contrived, and the beautiful sun and planet motion of Watt may be cited in evidence; but only a very slight examination of the contrivance is necessary to show that the crank, though disguised, was present, and that the success depended upon its presence. It was not a want of confidence in the crank which produced the sun and planet motion; it owed its contrivance simply to an invidious patent, and, as soon as that patent expired, the disguise disappeared, and the simple unincumbered crank assumed its place.

To see in what its superiority consists, let it be observed that, in the reciprocating piston, the following things take place: The piston is to be put in motion in one direction, then stopped; then put in motion in the opposite direction, stopped again; motion in the first direction begun, and once more made to cease. But these processes which produce the change of state from rest to motion, and from motion to rest, require time. Matter in motion acquires momentum, which must be gradually removed, otherwise these moving parts are subjected to concussion, as by the stroke of a hammer, and either suffers, or produces, injury. On the other hand, when brought to rest, matter cannot be instantly set in motion in the opposite direction, without a stroke and concussion equally violent. These effects, therefore, cannot be instantaneous; yet it is necessary that the motion which the steam gives off should be converted into continuous and uniform motion, while the parts of the engine itself must be allowed time to be brought to rest without shock, concussion, or jolt, and gradually and gently be again urged to their greatest velocity in the opposite direction. All this the crank effects with the utmost nicety of adjustment: it stops the piston as gently and softly as if a cushion of eider-down were placed to receive it; and after having brought it to rest, as gradually begins and accelerates its motion to its highest velocity in the opposite direction. An adjustment so complete is only possible in such a relation as that which subsists between the crank and the piston: the one describing uniformly the circumference of a circle, while the other moves by simultaneous graduations of alternately increasing and diminishing extent.

Now, comparing this mode of action with that of any of the sub

stitutes for the crank, by which it has been proposed to gain uniformity of power, we shall find that in these it would be required that the transitions from rest to motion, and from motion to rest, should be instantaneous, and hence such arrangements being soon disordered, have been speedily abandoned.

Perhaps one of the highest recommendations of a piece of mechanism is, that any very slight error in its construction shall not very materially affect its usefulness, nor any very slight derangement of its adjustment be attended with immediate deterioration; but that, on the other hand, the efficiency of the machine shall be consistent with such degrees of correctness of workmanship as can easily be accomplished, and such attention in superintendence as can readily be obtained; also, that the progress of disrepair shall be so gradual as to give timely warning of danger, and admit of ready repair and re-adjustment. The crank is precisely such a piece of mechanism; it possesses the property of reducing the errors of construction, arrangement, management, and adjustment, in a remarkable degree. This is well shown in respect to the valves. It is at the top and bottom of the stroke that the opening and shutting of these take place, and it is at these points that a minimum of the pressure on the piston is transferred to the crank, so that if the valves do not open with perfect precision, but either a little too late, or too soon, then will such error at that part of the circuit be comparatively harmless, for just then the motion of the piston is so slight that, through an arc of twenty degrees of the crank's orbit, it does not advance the hundredth part of the stroke, and therefore the effect of any error contained within that range will not affect the result of the crank by one-hundredth part of its full amount. Any error of adjustment is therefore diminished in effect to one-hundredth part of what would be produced, were the motion of the piston to be uniform, in portions corresponding to the arcs described, as would be the case in any other species of rotary conversion.

To this enumeration we might add other properties, but, perhaps enough has been said to remove the fallacy which would convert the simple crank into a destroyer of power. If so, our object is attained, and we shall be glad to find that the ingenuity and perseverance hitherto expended upon substitutes for the crank, are directed to more useful and rational purposes. A sufficiently wide field is open to improvement in the mechanical arts without interfering with a mechanism, which we have no hesitation in saying, is as perfect and simple in its nature as human ingenuity can make it.

Observations on M. Reiset's Remarks on the new method for the Estimation of Nitrogen in Organic Compounds, and also on the supposed part which the Nitrogen of the Atmosphere plays in the formation of Ammonia. *By* H. Will, *Ph. D.**

The method for the estimation of the nitrogen in organic substances

* Read before the Chemical Society, March 21, 1843.

described by Varrentrapp and myself* has been received by many chemists with the greatest approbation, as well on account of its simplicity as the accuracy and security with which results can be obtained. M. Reiset has however presented an essay to the Academy of Sciences at Paris,† in which he endeavors to prove by experiments at first sight very convincing, that the above-mentioned method is attended by two sources of error; the first of which in particular, were it true, would be quite sufficient to destroy completely the value of the method.

The cause of this first and principal source of error is, according to M. Reiset, that the nitrogen of the atmosphere forms a portion of the ammonia produced by the decomposition of nitrogenous matter by means of an alkaline hydrate, and that consequently too large an amount of nitrogen must always be obtained, particularly in bodies rich in carbon, bodies of difficult combustion, and those which readily form cyanogen compounds. This source of error becomes the more important, as from the experiments of Faraday,‡ which are confirmed by Reiset, it appears that by the fusion of many non-nitrogenous bodies with the hydrates of the alkalies a pretty considerable quantity of ammonia is formed. To those non-nitrogenous bodies which produce ammonia belongs in particular sugar, a substance which we proposed as an addition for the purpose of diminishing the violence of the absorption of the ammonia by the hydrochloric acid.

The numerous analyses of nitrogenous bodies made by Varrentrapp and myself, must have given a very considerable excess of nitrogen, if any formation of ammonia really took place, and were a constant source of error: it would be particularly evident in the analyses of ammeline, in which we mixed the substance with an equal weight of sugar. The accuracy and strictness of the results obtained by us from substances of well-known composition could therefore be ascribed only to accident, or perhaps to some source of error balancing the one just mentioned.

We thought we had met every objection of a source of error on this point by the experiments mentioned in our paper, in which we passed nitrogen and hydrogen gases through a glass tube over a red-hot mixture of carbonized bitartrate of potash and lime, or of pure charcoal soda and lime, and from which we did not obtain sufficient ammonia to be estimated as ammonio-chloride of platinum; and yet all the conditions necessary for the formation of ammonia and cyanogen were afforded in the mixture of soda, lime and carbon by the hydrogen becoming free from the combustion of the carbon at the expense of the oxygen of the hydrated water, as well as through the difficulty of its combustion.

M. Reiset appears to have overlooked the fact that finely divided carbon is also, as well as an organic substance, oxidized completely by means of the hydrates of the alkalies, and states that we have

* *Annal. der Chemie*, b. xxxix, s. 257. See also Philos. Mag. for March 1842, p. 216.

† *Compt. Rend*, vol. xv, p. 154; and *Ann. de Chim. et de Physique*, 3rd ser. vol. v, p. 469.

‡ Quarterly Journal of Science, vol. xix, p. 16; and Poggendorff's *Annalen*, vol. iii, p 455. [Also Phil. Mag. S. 1, vol. lxv, p. 309.]

neglected to prove in a satisfactory manner, that the facts observed by Faraday, according to which non-nitrogenous bodies, as sugar, acetate of potash, oxalate of lime, tartrate of lead, &c., by ignition with potash, soda, and hydrate of barytes, and access of air, give an appreciable quantity of ammonia, are without influence on the new process of analysis. He has undertaken this for us; and his experiments, which were made with stearine and sugar, gave him on combustion with soda-lime, under the same circumstances as in the execution of a nitrogen analysis, the following very remarkable results:—

Sugar.	Platinum obtained.	Nitrogen obtained.	Nitrogen in 100 parts.
0.250	0.02650	0.0038	1.52
0.500	0.05250	0.0075	1.50
1.000	0.0890	0.0127	1.27
1.500	0.104	0.0149	1.00
2.000	0.10725	0.0153	0.75

In these experiments the quantity of ammonia obtained was in exact proportion to the quantity of sugar employed, as far as *one* gramme; with *more* sugar, more ammonia was not obtained.

Reiset obtained further from 1 gramme stearine, 0.06475 platinum = 0.0092 nitrogen, and in two other experiments with sugar performed in an atmosphere of hydrogen (from 1 gramme,) 0.03375 and 0.034 platinum = 0.0048 nitrogen.

From both these last experiments, according to which non-nitrogenous bodies also eliminate ammonia in an atmosphere of hydrogen, M. Reiset concludes that the alkaline mixture possesses the property of condensing nitrogen so intimately and strongly that it cannot be expelled completely by a current of hydrogen passed over it for six hours, and that this state of condensation, approaching as it does the nascent state, makes the nitrogen more apt to enter into combination.

As a further proof of the incorrectness of our method, M. Reiset brings forward the analysis of cinchovatina, an organic base discovered by Manzini in Jaën Cinchona, from an analysis of which, performed with a mixture of sugar, almost 5 per cent. more of nitrogen than the calculation required was obtained. 0.052 cinchovatina gave, namely 0.949 ammonio-chloride of platinum = 11.95 per cent. nitrogen.

The calculation from the formula $C_{46}H_{27}N_2O_8$ gives only 7.16 per cent.

The excess of 4.8 per cent. here obtained, estimated by weight, amounted to 0.024 gramme nitrogen, or in volume nearly 25 cubic centimetres; in the above experiments with sugar 0.015 gramme of nitrogen was, according to M. Reiset, condensed in the soda-lime, and therefore took a part in the formation of the ammonia.

If we consider that the decomposition of organic bodies of difficult combustion by the hydrates of the alkalies does not take place at a heat below redness, that further, the heating of the contents of the tube by the fire placed around it cannot be so sudden as to produce in an instant the temperature necessary for combustion, but that the heat, even when sudden, penetrates the mixture only by degrees, and that the greater portion of the inclosed or condensed air is driven out

by its own expansion, we can scarcely comprehend how M. Reiset could entertain the idea that the nitrogen condensed in the mixture could take a part in the formation of ammonia. He certainly brings forward an experiment apparently supporting this view, viz. that by the combustion of 1.500 gramme of sugar in a current of air, the combustion being thus quickened, only 0.0099 nitrogen was obtained, instead of 0.0149. The ammonia did not increase when pure nitrogen was passed over the mixture during the combustion. I shall subsequently return to this point.

I have repeated and partly varied the experiments of Reiset, and have come to entirely different results.

1.214 sugar-candy of the shops by combustion with the usual mixture of soda-lime, which had not been previously ignited, gave on evaporation with chloride of platinum and ignition of the washed residue, 0.006 metallic platinum = 0.00086 nitrogen = 0.07 per cent. of the sugar burned.

0.386 pure stearic acid recrystalized from alcohol, gave 0.002 metallic platinum = 0.00028 nitrogen.

0.430 leguminous starch, prepared in the laboratory of Giessen, and purified with sulphuric acid, gave 0.005 metallic platinum equivalent to 0.0007 nitrogen.

A gramme of the above-mentioned starch was submitted to dry distillation. The product of distillation was mixed with hydrochloric acid, evaporated, the residue dissolved in water, mixed with chloride of platinum, and again evaporated. After treatment with alcohol and æther, a portion of ammonio-chloride of platinum remained, which ignited left 0.004 metallic platinum. The ammonia obtained by the combustion with soda-lime was thus, in part at least, contained in the starch, and was no product of the operation.

In both the following experiments, conducted exactly as an ordinary combustion, I employed soda-lime ignited just before its introduction into the tube.

1.000 gramme stearic acid decomposed with soda-lime in a tube 1½ foot long and half an inch wide, left, after evaporation to dryness with chloride of platinum and resolution in æther-alcohol, no visible trace of ammonio-chloride of platinum.

2.000 grammes pulverized metallic tin, after ignition with soda-lime and treatment of the residue after the evaporation of the hydrochloric acid with chloride of platinum, afforded an extremely small quantity of yellow powder, which possessed all the properties of ammonio-chloride of platinum.

In the following experiments, a stream either of atmospheric air or of nitrogen was passed through the tube during the successive oxidation of the substance, by means of an alkaline hydrate. Both the atmospheric air and the nitrogen were dried by means of sulphuric acid, which had been freed from nitric oxide by treating with sulphate of iron.

The volume of gas passed through was about from three to four thousand cubic centimetres, and the combustion throughout the whole length of the tube was so conducted, the experiment lasting from two

to three hours, that the conditions necessary for the formation of ammonia were given at every moment.

4.000 grammes perfectly pure recrystalized sugar, ignited in a tube 2½ feet long, with a large mass of soda-lime in a current of air, gave no trace of ammonio-chloride of platinum.

20 grms. of common pulverized tin, oxidized in the same way with soda-lime, gave a quantity of yellow powder, too small to be weighed. The uninterrupted disengagement of hydrogen proved, however, that the tin was oxidized at the expense of the alkaline hydrate.

4.300 grammes of recrystalized sugar were introduced by degrees through the tubulure of a retort whose neck was obliquely turned up, and in which soda-lime was in a state of fusion. An aspirator was attached to the absorption apparatus connected with the retort, so that the gaseous product formed, following the current of air, should pass through the hydrochloric acid. Only an extremely small trace of ammonio-chloride of platinum was obtained. The same experiment repeated with tin, zinc, and pure metallic iron, always afforded ammonio-chloride of platinum, yet so slight a trace that in most cases it did not admit of being estimated.

When hydrate of potash was employed instead of hydrate of soda, I always obtained potassio-chloride of platinum, because, from the violent evolution of the hydrogen, portions of the alkali were driven over into the hydrochloric acid. In another experiment 20 grms. of metallic tin were melted with fresh fused hydrate of soda in a thin U-formed tube, so that, during the continuance of the experiment, a fresh quantity of air was always brought into contact with the nascent hydrogen; I obtained thus 0.008 ammonio-chloride of platinum = 0.00057 nitrogen. In an experiment in which nitrogen was used instead of atmospheric air, a similar result was obtained, namely, 0.007 ammonio-chloride of platinum.

These experiments prove that the nitrogen of the atmosphere can in no way form ammonia with hydrogen in a nascent state. The extremely small quantities obtained in most cases, must, consequently, have some other source, which it is very difficult to avoid. This, however, may be attained by the following method:—

Hydrate of soda was melted in a silver crucible until it became liquid, and then mixed with a small quantity of pure iron, reduced from the oxide by means of hydrogen. This was readily oxidized with disengagement of hydrogen gas; it was then poured into a silver dish, previously warmed, and, after it had cooled, was broken into pieces, and introduced into a slightly curved tube of hard glass, half an inch in diameter, previously ignited; from 4 to 5 grammes of pure iron, reduced from the oxide by hydrogen, were then immediately added; the tube was heated by charcoal placed under it, and nitrogen, or atmospheric air, passed through it. On the first passage of the air, an extremely small quantity of ammonia was generally detected by means of dahlia-paper, or by a rod moistened with dilute hydrochloric acid; but this disengagement of ammonia was only observed for a short time, and always ceased before the evolution of the hydro-

gen, from the oxidation of the iron, began. When this took place, it was connected with the absorption apparatus, and the alkali kept in a state of fusion, until all the metal was oxidized. By carefully following this plan, I never obtained ammonio-chloride of platinum.

The same experiment was repeated, with a like result, with perfectly pure crystalized tin, as it is easily obtained when a polished rod of tin is suspended in a vessel in which water, with a little hydrochloric acid, rests on a concentrated solution of tin; after one or two days, a splendid crystalization forms. If the metal happened to be touched by the fingers, or allowed to remain exposed to the air before the experiment, a disengagement of ammonia invariably occurred; but not when it, as well as the alkaline hydrate, were fused just before being employed. Pure tin is with great difficulty oxidized by hydrate of soda, and must be kept in a state of fusion with it for many hours before the oxidation is complete.

Reiset states that ammonia is disengaged by heating metallic iron and potash-ley to 292° Fahr., with access of air, but not so in an atmosphere of nitrogen. This statement rests on a very equivocal foundation. Pure iron can be heated for a long time in a boiling potash-ley without the disengagement of hydrogen; the oxidation takes place only on the fusion of the alkaline hydrates. If a quantity of potash-ley, which has stood for a long time in a perfectly clean retort, be heated, there is always observed, at the commencement, a slight disengagement of ammonia, but this soon ceases altogether.

The, by no means inconsiderable, quantities of ammonia obtained by Reiset, admit of no other explanation, than that his mixture of soda and lime contained a nitrate, probably nitrate of potash, which, as Faraday states, easily evolves ammonia when the smallest trace of it is melted with zinc and an alkaline hydrate. If Reiset had only, in a small degree, followed or observed the extremely cautious and circumspect manner of proceeding of that celebrated English philosopher—a manner which is with justice admired by him—he would not have endeavored to find sources of error in a method to which, on this point at least, no very weighty objection can be made.

The nitrate of potash contained in the soda-lime used by Reiset, was very probably owing to the circumstance that most manufacturers add a little of it to the commercial hydrates of soda and potash, for the purpose of improving their appearance. In Reiset's experiment, where he obtained 4.8 per cent. too much nitrogen in chincovatina, his mixture must have contained very nearly ½ per cent. of nitrate of potash, when it is considered that his tube contained from 50 to 60 grammes. This also explains, in a much simpler and easier manner, the formation of ammonia in an atmosphere of hydrogen, and also the limited increase of ammonia from the increased quantities of sugar employed. As the whole quantity of nitrate of potash would be destroyed by from 1 to 1½ gramme of sugar, the quantity of ammonia could not increase. The nitrogen was here certainly contained in such a condensed state, that a stream of hydrogen gas, passed over it during twelve hours, did not expel it.

I now come to the second source of error objected, by M. Reiset,

to the new method. It appears, from his statements, that a part of the chloride of platinum is reduced to protochloride when the hydrochloric acid fluid, which in many cases contains liquid hydro-carburets, is evaporated to dryness with it in a water-bath; consequently, too much nitrogen must always be obtained, as this protochloride of platinum is insoluble in æther and alcohol. And this source of error has the more injurious effects on the result, the more its necessary conditions are afforded; and these conditions are the blackening of the hydro-carburets by the hydrochloric acid. In a direct experiment with sugar, made for this purpose, and in which the burning was managed in such a manner that the hydro-carburets, being produced at a low temperature, floated in abundance on the hydrochloric acid, on evaporation in a water-bath no reduction of the chloride of platinum could be observed. It must be allowed that, in such a trifling case of occurrence, the result would not be affected by it. Indeed, the formation of hydro-carburets, easily decomposable by hydrochloric acid, may be completely avoided by keeping the nearer end of the tube pretty strongly ignited, as the hydro-carburets are the more constant when produced at high temperatures.

The highly remarkable and accurate experiments of Faraday on the disengagement, or formation, of ammonia by the fusion of hydrate of potash with a metallic, or a non-nitrogenous, body, a result which I have also found in all my experiments, (but which was so trifling that it could not be attributed to any part played by the nitrogen of the atmosphere,) as also the investigation of Professor Liebig on the ammonia contained in rain-water, gives a complete and simple solution of the question, from whence comes this disengagement of ammonia, so often observed, and so difficult to be avoided?

The experiments of Faraday go entirely to show that there is some unknown source of ammonia, and that the nitrogen of the atmosphere in his experiments played no actual part; they are so convincing, and made without any preconceived opinion, that I cannot refrain from giving a short extract from them here. They prove, as it appears to me, directly the reverse to the conclusion which M. Reiset has drawn from them, and are of the greatest importance in the question, whether the nitrogen of the atmosphere plays a temporary part in the formation of ammonia by the decay of organic matter, or by the oxidation of metals, with or without the disengagement of hydrogen? An affirmative or negative to this question has a very important influence on the theory of the nutrition of plants.

Faraday observed that an organic substance, the quantity of whose nitrogen he wished to estimate, yielded ammonia by fusion with hydrate of potash, although he obtained none when it was heated alone in a tube. By extending his experiments further, he found that many non-nitrogenous organic bodies, as also many metals, presented this phenomenon, as, for instance, iron, zinc, tin, lead, arsenic, and also copper. He obtained, for example, a very perceptible quantity of ammonia with woody fibre, oxalate of potash, oxalate of lime, tartrate of lead, acetate of lime and asphaltum; with acetate of potash, acetate and tartrate of lead, tartrate and benzoate of potash, oxalate

of lead, sugar, wax, olive oil, and napthaline, very little; and with resin, alcohol, æther, and olefiant gas, none whatever. The quantity of ammonia agreed, in a remarkable manner, with the quantity of hydrate of potash used in the experiment. He further observed, that perfectly pure hydrate of potash, evaporated so far that it ceased to give off water, when heated alone yielded no ammonia, but that it acquired this property when exposed to the air for some time. He observed exactly the same with caustic lime, and hydrate of lime, and also with fresh prepared potash-ley, allowed to stand for twenty-four hours.

Faraday further obtained ammonia when he heated a strip of well purified zinc with hydrate of potash, made from potassium, in a carefully prepared atmosphere of hydrogen; but could discover no ammonia when he heated the zinc with hydrate of potash which had been previously kept in a state of fusion until it ceased to give off water. He states, moreover, that the ammonia was generally observed before the disengagement of the hydrogen, by the decomposition of the substance employed, commenced.

Tartrate of lead, ignited with potash and the cold residue, brought in contact with a drop of water, evolved ammonia.

White clay from Cornwall, which, after being strongly ignited, was exposed to the air for eight days, yielded much ammonia; while another exactly similar portion of the same clay, which, after ignition, was preserved in a well-stoppered bottle, gave no ammonia.

Pure sea sand, heated to bright redness in a crucible, and cooled on a plate of copper, gave no trace of ammonia, although it was very readily observed when the hot sand, previous to its being heated, was held for some moments in the hand, and stirred about with the finger.

These experiments evidently agree with the observations of Braconnot,* who states that many porous minerals, such as trap from Chaume de Tendon, eurite, some species of granite, serpentine from the Vosges, amphibole, muschelkalk, &c., by distillation in a glass retort, yielded an ammoniacal product.

The experiments of Faraday show with the greatest accuracy that the ammonia was not only not formed, but that it either existed already in the material employed, or received it from the air by exposure. The quantities obtained were so extremely small that he could not estimate them.

In the foregoing experiments, I have not only confirmed, but, at the same time, demonstrated the correctness of, Faraday's statement, that the nitrogen of the atmosphere does not in any way possess the property of forming ammonia with hydrogen at the moment of its separation from any combination. If this were the case, a quantity of ammonia capable of being estimated, and in proportion to the duration of the experiment, or the quantity of the material, would have been obtained in the experiments with tin, iron, and sugar—in which, by the gradual heating of the substance with an alkali in a continued

* Annales de Chimie et de Physique, t. lxvii, p. 104.

current of air, or of nitrogen, the conditions for the formation of ammonia were as favorable as possible throughout the whole combustion; but this did not occur, and, by proper care, we are even in a condition to avoid every trace of ammonia, although nascent hydrogen may come in contact with nitrogen gas.

If we consider that ammonia forms a never-failing constituent of our atmosphere—that, further, it is a body which is easily absorbed by liquid and porous substances, particularly when these latter possess, at the same time, the properties of an acid,—we must at once perceive that, being in possession of an exceedingly delicate test of the presence of ammonia, that volatile alkali must be found in all, or nearly all, substances exposed to the air.

It is quite evident from this why Faraday did not obtain ammonia with fresh hydrate of potash which had been previously melted, nor with resin, which is not a porous body, although resin, like other organic bodies, was decomposed with the disengagement of hydrogen gas by fusion with the hydrate. A small quantity of nitrogen, contained in the body as a constituent, may be in part, or altogether, the cause of the disengagement of ammonia in many cases where Faraday observed it. The fact that the flocculent black residue, always obtained by the solution of zinc in sulphuric acid, after being well washed, disengages a pretty considerable quantity of ammonia, accounts very easily for the presence of nitrogen in commercial zinc. Cast iron, according to Schafhaeutl, also contains nitrogen.

The statements contained in most treatises on chemistry, that iron, by its change into oxide, under the combined influence of moisture, and air containing carbonic acid, affords the nitrogen of the latter the conditions necessary to form ammonia, agree exactly with the above cases of its supposed formation. This production of ammonia, if it actually took place, presupposes that iron is capable of decomposing water, with the disengagement of hydrogen gas, at the common temperatures, which is by no means the case; it presupposes further, that the hydrogen, on being set free, possesses a far greater affinity for the nitrogen, than for the oxygen, of the atmosphere, which completely contradicts our general experience. At high temperatures, where water would be decomposed by iron, ammonia is not formed. Kuhlman* obtained only hydrogen and nitrogen, but no ammonia, by the passage of steam and nitrogen over pyrophorous iron heated to a strong red heat.

I have repeated the doubtful experiment of Austin, (at least according to the result of Hall,†) in such a manner that the ammonia in the atmosphere (but not its carbonic acid) was, as perfectly as possible, shut out. I introduced into a flask, of from four to five litres capacity, some iron nails, (one pound,) previously cleaned from all oxide by dilute hydrochloric acid, and then well washed with pure water, and also sufficient distilled water to cover the bottom. The flask was connected, air-tight, by an intermediate tube, with a second smaller

* Abhandlung ueber die Salpeterbildung: Annal. der Chem. und Pharm., Bd. xxix, S. 285.

† Ann. de Chim. et de Phys., t. ii, p. 42.

one, which contained a small quantity of very dilute muriatic acid. By a second hole bored in the cork of the small flask, a tube containing asbestos, moistened with pure sulphuric acid, was attached, and through which the external air communicated with that contained in the greater flask. The object of the hydrochloric acid was to prevent the ammonia formed from passing into the sulphuric acid. The air was renewed every day in such a manner, through a second tube, closed with wax, in the cork of the first flask, that the air entering must pass through the sulphuric acid tube.

After from fourteen to eighteen days' oxidation, the oxide, of which a considerable quantity had already formed, was washed out of the flask with water and a little dilute hydrochloric acid; dissolved in hydrochloric acid, and the solution, to which chloride of platinum was added, evaporated nearly to dryness in a water-bath. The residue dissolved completely in æther-alcohol, and did not deposit a trace of ammonio-chloride of platinum even after standing for twelve hours, nor could any ammonia be found in the muriatic acid contained in the small flask. If the iron was here oxidized at the expense of the water, and if the hydrogen by that means set free had, at the moment of its disengagement, formed ammonia with the nitrogen of the air, nearly three grammes of ammonio-chloride of platinum would have been obtained for every gramme of the oxide treated in the above manner. This is a quantity which could not escape observation.

There is no doubt from this that the ammonia observed in the rust of iron was obtained from the atmosphere.

Herman,* in an essay "On the decay of wood," mentions an experiment in which the nitrogen of the atmosphere was directly absorbed, and partially converted into ammonia, by the decay of fresh wood. Herman found nearly one-third part of the nitrogen, which, according to his experiments, existed as a constituent of the wood, in the products of its decay. He concludes from this that two parts escaped in the form of ammonia.

The most perfect process of decay with which we are acquainted, is the production of acetic acid from alcohol. If the nitrogen of the atmosphere possessed the property of taking part in these metamorphoses, instead of pure acetic acid, an ammoniacal salt of it would be obtained in the quick process for the manufacture of vinegar, where the woody fibre undergoes a slow decay with the alcohol. As yet, however, no ammonia formation has been observed.

The process of decay of organic substances which contain little, or no, nitrogen at the surface of our planet, is as old as the occurrence of living matter upon it; it is so general and everywhere perceptible, that our atmosphere would soon be poisoned with ammonia, there being no such chemical attractions for *nitrogen* gas (as an element) as for oxygen; and its amount of nitrogen would certainly have decreased, if this most indifferent of all gaseous elements possessed the property of contributing, as such, to the formation of ammonia.

Lond. & Edinb. Philos. Mag.

* Journ. für Prakt. Chemie, Bd. xxvii, S. 165.

On a new Method of obtaining pure Silver, either in the metallic state, or in the form of Oxide. *By* WILLIAM GREGORY, M.D., F. R.S.E., Member of the Chemical Society, &c.

The chemist, as well as the metallurgist, has frequent occasion to purify silver, especially from copper, which is dissolved along with it by nitric acid, the proper solvent of silver. By converting the silver into the insoluble chloride, it is effectually purified from copper, as well as from all other metals, the chlorides of which are soluble. But here the difficulty begins: the chloride of silver is a very unmanageable product, at least in the moist way. It is true, that, if placed in water acidulated with hydrochloric acid, in contact with zinc, or iron, the chloride of silver is reduced. But the process is tedious, seldom complete, and, in the end, unsatisfactory; for some zinc adheres to the reduced silver, so that it is not removed by digestion with moderately strong hydrochloric acid. This is proved by the action of ammonia, which extracts a good deal of oxide of zinc. Moreover, the zinc, or iron, is hardly ever pure; and its impurities, arsenic, carbon, and perhaps also copper and tin, remain with the silver. I have never got from silver, thus reduced, a colorless solution of nitrate.

It is no doubt better to decompose the dried chloride of silver by the action of carbonate of potash, or soda, at a red heat. But, although the silver is thus obtained pure, the process requires much experience and dexterity. If the heat is too low, the reduced silver is disseminated in small globules through the mass; if too high, the alkali corrodes the crucible rapidly, and the contents fall into the fireplace, or ash-pit. There is often, also, a portion of silver cast up by the effervescence on the sides of the crucible, in small globules, which do not readily run down into the fused mass below. In short, this process, always ticklish, often fails. It is therefore desirable, if possible, to dispense with a furnace heat.

The method of reducing the silver from the impure nitrate by protosulphate of iron, does not answer. It is long ere the action is terminated, and, besides, some sulphate is always formed, which *is not reduced*, and is partly precipitated with the metal, and partly retained in solution.

The only remaining method, known to me, is that of reducing the silver from the impure (cupreous) nitrate, or sulphate, by means of copper. The chief objection to this method is, that it is somewhat tedious; but it is also not improbable, that a trace of copper may adhere to the silver, chemically combined, as is the case, to a large extent, with mercury in the *Arbor Dianæ*. At least, I have generally found copper in the silver I have thus prepared.

Considering these things, it appeared desirable to have, once more, recourse to the chloride, which can be easily obtained perfectly pure, and to decompose it without the contact of any reguline metal. The most obvious plan was to try the action of caustic alkalies in the moist way, and, although it has been, singularly enough, hitherto overlooked, I find that caustic potash may be used with complete success.

Diluted potash, or even a concentrated solution, if cold, has no apparent action on chloride of silver; and this, I presume, explains how the reaction about to be described has not been noticed. But a solution of potash, spec. grav. 1.25 to 1.30, with the aid of heat, decomposes almost instantaneously the moist chloride of silver, converting it into a heavy, fine, jet-black powder, which is pure oxide of silver. This oxide dissolves without the smallest residue, and without effervescence, in diluted nitric acid, and yields a colorless and pure nitrate. The heat of the spirit-lamp reduces the oxide to a coherent, spongy, mass of absolutely pure silver.

The following method appears to me the most advantageous:

The cupreous solution of silver is precipitated by common salt, while hot, and the chloride of silver well washed by decantation with hot water. It should also be broken down with a spatula of platinum, or a glass rod, during the washing, but not ground in a mortar, which causes it to cake, and impedes the action of the potash. The chloride, *while still moist*, is covered to about half an inch with a solution of caustic potash, spec. grav. 1.25 at least, and then boiled. During the boiling, which is best performed in a capsule of clean iron, silver, or platinum, the chloride is to be well stirred, in order to bruise all curdy, or lumpy, particles. In five or ten minutes the powder has become black. If a small portion, taken out and washed, do not dissolve without residue in dilute nitric acid, the potash is to be decanted off, and the powder, still moist, is to be well rubbed down in a mortar, which may *now* be done with advantage. It is then returned into the capsule, and again boiled for five minutes with the same, or with fresh, potash. It will now dissolve entirely in nitric acid; but, if not, a second grinding will infallibly succeed. It is now only necessary to wash the oxide, which is completed by decantation in a few minutes, as the powder, from its great density, sinks at once to the bottom. The first two or three washings are made with hot water, the remainder with cold water; for, when the oxide is nearly washed, it rises partially to the surface *with hot water*, and thus a loss is occasioned in decanting. Of course, the whole washings (except the first, owing to the strength of the potash,) may be conducted on a filter. But the powder is so fine, that probably a good deal would adhere to the paper when dry.

This oxide of silver appears in a form quite distinct from that of the oxide precipitated by potash from the nitrates, and is hitherto undescribed. It is very dense, homogeneous, and has a pure black color, which has, if anything, a tint of blue; whereas the common oxide is bulky, far less dense, and of a grayish-brown color. They appear, however, to be chemically identical. Not having a microscope, I have not studied their physical characters minutely; but I suspect, from its aspect in the liquid in which it is formed, that the new oxide is crystaline.

It is obvious that the above process furnishes an easy method of procuring a very pure oxide of silver, and, of course, the action of heat gives us the silver in the state of metal. It is, I conceive, applicable both to the manufacture of nitrate, (in a state of absolute pu-

rity,) and to the metallurgic process for obtaining pure silver. For both objects, it is a matter of no consequence if some chloride should have escaped the action of the alkali. This chloride is left undissolved by the nitric acid, and is separated by filtration; while, if the oxide (not quite free from chloride) be mixed with a little nitre, or carbonate of potash, and fused, the whole silver is obtained with the utmost facility.* In order to give an idea of the ease with which the whole is performed, I may mention that I dissolved a half-crown, and obtained the whole of the silver it contained, within a very trifling fraction, (chiefly decanted in the *first* washing of the chloride, *but not lost*,) by the above process, *within two hours*, in a fused state. The silver was quite pure. There is no doubt that to chemists, also, an easy method of obtaining quickly pure oxide of silver, in a form much less hygrometric than the usual one, will be acceptable.

It is particularly to be noticed, that, if the chloride have ONCE BEEN DRIED, it is with great difficulty decomposed, even by a long boiling with potash.

King's College, Aberdeen, Jan. 20, 1843. Ibid.

The Hot Blast Patent.

In the House of Lords, on Monday, March 6th, before the Lord Chancellor, and Lords Brougham and Campbell, the case of the "Household Coal and Mining Company *vs.* Neilson and others," came up for decision.

The appellants in this case were the defendants in an action tried before the Court of Session in Scotland, for an alleged infringement by them of the hot blast patent, when a verdict was found against them; and came now before the House of Lords on a bill of exceptions, tendered by them against the charge of the learned judge, (Lord Justice Clerk,) who presided at the trial.

The Lord Chancellor. My lords, the principal question in this case arises out of the eleventh exception. The learned judge (who presided on the trial) stated to the jury what he considered to be sufficient evidence to support prior use, so as to invalidate the patent. The learned judge expressed himself in these terms. He says: "You will observe that it is settled that the trials founded on as a proof of prior use, must have been public—must have been continued, not abandoned—must have continued to the time when the patent was granted—I do not say to the very exact period, but it must have been known and used as a useful thing at the time." (After some observations on the meaning of the word "trials" as used by the presiding judge in the jury court, the Lord Chancellor continued—I understand the proposition of the learned judge to be this—that if the machine had been made, and had been put in trial, unless those trials had gone

* In fact, this process, imperfectly performed, is an excellent preliminary step, when a large quantity of chloride is to be reduced. The impure oxide requires so little alkali to complete its decomposition, that the crucible runs no risk. A little borax may be added as a flux.

on, and the machines had been used, up to the time of the granting of the letters patent, it would not be evidence of prior use so as to invalidate the letters patent. Now, I am obliged to say, with all deference to the learned judge, and with all respect to the learned judges of the Court of Session, that I think in that respect they are mistaken, and that, if it is proved distinctly that a machine of the same kind was in existence, and was in public use; that is, if use, or if trials, had been made of it in the eye, and in the presence, of the public, it is not necessary that it should come down to the time when the patent was granted. If it was discontinued, still that is sufficient evidence in support of the prior use, so as to invalidate the letters patent. If it is discontinued, provided it has been once in public use, and the recollection of it has not been altogether lost—if it has been once publicly used, it will be sufficient to invalidate the letters patent, although the use may be discontinued at the time when the patent was granted. Now, my lords, I apprehend that that is the law, and the known law, upon the subject in this country. I never heard it before questioned that the notorious public use of the invention, before the granting of letters patent, though it may have been discontinued, is sufficient to invalidate the letters patent. Then, my lords, the remaining question for consideration is this, and it is an important one, whether, if the learned judge laid down the law incorrectly to the jury, this was calculated to mislead the jury? (His lordship then explains how it was calculated to mislead, and says)—Therefore, it is perfectly obvious, that, if the learned judge be incorrect in the manner in which he stated the law, in the particular in which I have stated, it was calculated to mislead the jury. Under these circumstances, my lords, I should recommend your lordships to allow the eleventh exception, and to disallow all the rest.

Lord Brougham. My lords, I entirely agree in the view taken, and for the reason so luminously expressed, by my noble and learned friend on the woolsack. If we are of opinion, first, that the law has been mistaken, and, under a misapprehension of it, it has been erroneously delivered by the judge to the jury; and if we are, secondly, of opinion that the misdirection in point of law, the mistake in point of law, committed by the learned judge, had a direct tendency, I may almost say an inevitable tendency, to mislead the jury in the conclusion to which they should come, and in the verdict which they should render; then, my lords, both of these questions being answered in the affirmative, that the law was mistaken, and that the mistake tended to mislead the jury in their verdict, we have no choice, but must allow the exception. Now, my lords, a more important mistake in point of law, your lordships will give me leave to say, could not possibly have been made by the learned judge, than that into which the learned judge fell upon the present occasion. And I will not allow it to be said for one moment, in dealing with this question, that there is anything doubtful, that there is anything speculative, that there is any new law to be laid down, or even any new topics in respect of the law about to be broached here, in dealing with the direction of the learned judge; for I speak with all possible respect for that learn-

ed judge's great ability and experience in his profession in Scotland, when I say that this law, which has been mistaken here by his lordship, is a matter of as perfect certainty, as thoroughly known, and as little drawn into doubt, in Westminster Hall, where the law is administered touching the construction of the statute of James, the Patent Act, as any one branch of the law most commonly known, and most frequently administered, by our courts. It is one of the greatest errors that can be committed, in point of law, to say that, with respect to such an invention as that, it signifies one rush whether it was completely abandoned, or whether it was continued to be used down to the very date of the test of the patent, provided it was invented and publicly used at the time, twenty or thirty, or as, in this case, forty, years ago, it is perfectly immaterial; there being, in my apprehension, no kind of doubt that the jury would say—"Why should we consider whether it was used at the Bradley Works, or not? Why should we consider whether it was a trial, or a completed invention? Be it so that it was used forty years ago—be it so that it was a complete invention; we hear the learned Lord Justice Clerk telling us that we need not trouble ourselves upon these facts, for it is enough for us if it was abandoned, and that takes the facts out of the case, and leads us to find a verdict the other way." Upon these grounds, my lords, we have no choice in this application, it being a bill of exceptions; we have no hesitation in saying that the law was misconceived, and misstated to the jury. The law is undeniable, it is a matter of no doubt or hesitation with any man in this country who has been accustomed to administer it, or, I will venture to say, with any practitioner whose opinion is entitled to any weight; and I am also of opinion that the law so laid down tended to mislead, and must necessarily have tended to mislead, the jury. Upon these grounds, I have no hesitation in supporting the proposition of my noble and learned friend, that the eleventh exception must be allowed.

Lord Campbell. The only question is this, whether this misdirection shall be considered as immaterial? When I look at the form of the issue, I cannot say that it was immaterial, because the issue is, "whether the invention, as described in the said letters patent and specification, is the original invention of the pursuer." Now, you cannot say that it was the original invention of the pursuer within the meaning of the issue, if it had been publicly known and practised by others before the patent was granted. It has been said that there was no evidence; but I think that is a mistake—what conclusion the jury have come to I know not—but at the Bradley Iron Works there was such a machine, as Mr. Rutherford acknowledged at the bar, as would have amounted to an infraction of the patent, if the use of it had been subsequent to the patent. Then, that being so, I know not what conclusion the jury may have arrived at. They might have thought that this was a perfect machine, that it was the same machine, and that it had been publicly used. If they had been of that opinion, although it had been abandoned, they ought to have found a verdict for the defendant. Under the circumstances, I regret exceedingly that I am obliged to concur in the opinion that has been

expressed by my noble and learned friends, that this eleventh exception must be allowed; and the consequence of that will be, that there must be a *venire facias de novo*, and that the case must be tried by another jury. Lond. Mech. Mag.

On a Method of Registering the Force actually transmitted through a Driving Belt. By EDWARD SANG, Esq., F.R.SS.A., *Professor of Civil Engineering, College, Manchester.*

It is a desideratum to have the means of ascertaining how much force is actually consumed in the working of a machine. Whenever the motion is communicated by the intervention of a belt, or band, this can be very easily accomplished.

When we see a belt passed over two pulleys, and look without any narrow examination at the motion, we regard the action as a very simple one; there is more in it, however, than appears at first sight. For the sake of clearness, let us call the driving pulley the drum, and the other the pulley. The belt passed over them, whether plain or crossed, has two free parts, one of which *draws*, and the other *follows*. If it were possible that no force were needed to turn the pulley, these two free parts would be in the same state of tension; but whenever any resistance is made to the motion of the pulley, the drawing part is distended more, and the following part less, than usual; and experiments show that, within all practical limits, *this change is exactly proportional to the pressure necessary for overcoming the resistance.*

As the movement proceeds, the distended part of the belt is lapped over the drum, and, so to speak, the contracted part is lapped over the pulley, so that the circumference of the drum moves more swiftly than that of the pulley; thus, if the distension be 1 in 100, for 100 inches of the drum there would only be 99 inches of the pulley passed over.

The difference between the velocity of the drum and that of the pulley, thus indicates the pressure needed to carry the drum round. Now, this pressure, combined with the distance through which it acts, gives the force used; and hence the simple difference between the distances passed over by the circumference of the drum, and by that of the pulley, is exactly proportional to the force; *and we have only to contrive some method of registering this difference, in order to have a record of the total force transmitted by the belt.*

There may easily be contrived a variety of arrangements for showing the difference between the motions of the drum and pulley. Thus a pair of indicators may be fitted, one to each shaft, so as to tell the total number of turns made by each; from this number, by help of the measured diameter, the distance passed over by each circumference can be found, and thus the element for knowing the force transmitted can be had.

Or, otherwise, and this, perhaps, is the most convenient arrangement, a light pulley, having its circumference one foot, may be

brought to bear against the belt on the drum, and another against the belt on the pulley; if these light pulleys have counting gear attached, a simple reading off and subtraction will give the difference of distance.

Having now ascertained the difference between the motions of the drum and pulley, it remains to ascertain by what this must be multiplied, in order to give the force. It is not my object, at present, to enter into the theory of the matter—although this theory presents several points of considerable interest—but to give a practical application of the principle. In order to find out the force due to a single foot of difference, we have to run the pulley unburdened for a considerable time, taking notice of the difference of motion, and then loading the shaft by means of a spring friction-strap with two arms, repeat the observation over as many strokes of the engine, or turns of the drum; in this way we shall have a new difference, and, subtracting the one from the other, we shall have what is due to the force as shown by the friction strap.

When the multiplier for one belt has been ascertained, that for any other belt may be approximately computed, if it be of the same material, by having regard to the relative weights of a foot of each; so that a pair of accurately constructed counters form a portable apparatus, by means of which the force transmitted by any belt may at once be ascertained, the weight, length, and material of that belt being known.

Manchester, Sept. 1, 1842. Edinb. New Philos. Jour.

Solid and Hollow Axles.

A paper, by Mr. J. O. York, who has a patent for hollow axles, was read at a late meeting of the Institution of Civil Engineers, giving an account of some experiments which he has made for the purpose of testing their strength, as compared with solid axles. The paper described the common causes of fracture, attributing it to the concussion and vibration produced by various circumstances, such as a bad state of the line, the sudden opposition of any obstacle on the rail, or the shocks arising from the wheels striking upon the blocks, or the chairs, when thrown off the line. These shocks, which it was impossible to calculate the extent of, it was contended, should be provided for by axles which would bear a series of heavy blows without fracture. The force of vibration, and its tendency to produce fracture in rigid bodies, was then treated of, with its effect in destroying the most fibrous texture of iron where elasticity was prevented, as is the case with railway axles, comparing the action with that upon the axles of ordinary road carriages, where the concussion was reduced by an elastic medium, such as the wood spokes of the wheels, which were bad conductors of vibration. By calculation it was shown that the twisting strain arising from the curves of the railway was of too small an amount to be considered as a cause of destruction to the wheels, or axles, even on lines with curves of short radii; and it was submitted that the requisite qualities in railway axles were—first, the

greatest possible degree of rigidity between the wheels, to prevent the axle from bending, or breaking, from concussion; and, secondly, the greatest quantity of elasticity and freedom in the particles of iron within the axle itself, to prevent the injurious effects of vibration. It was contended that the hollow axle was better able to resist these strains than a solid one, because the comparative strengths of axles are as the cubes of their diameters, and their comparative weights only as their squares; consequently, with less weight in the hollow axle, there must be an increase of strength, and also that the vibration had a free circulation through the whole length of the hollow axle, no part being subject to an unequal shock from the vibration, and that the axle would therefore receive less injury from this cause than a solid one.

A long series of experiments, which had been made in the presence of Major General Pasley, and numerous engineers, was then read, and showed results confirmatory of the position assumed by the author of the paper. In the discussion which ensued, it was allowed that, theoretically, the hollow axles must be stronger than the solid ones, inasmuch as the same weight of metal was better distributed; and the practical experiments fully bore out the theory. Some curious specimens of solid axles, which had borne a great number of blows before breaking, were exhibited by the Patent Axle Company, from Wednesbury. The quality of the iron was excellent, and, had the same material been manufactured into hollow axles, it was agreed that many of the melancholy accidents upon railways would not have occurred.

Lond. Mech. Mag.

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*Accidents from Broken Axles Prevented.**

We have been favored with an inspection of an ingenious method of preventing a carriage falling in case the axletree should become fractured, patents for which have been secured for Great Britain and the continent. The plan is simple, but effective. Two false axles, of sufficient strength, are placed one before and one behind the axle of the carriage, and connected at each end by a box of metal large enough to take in the nave of the wheel; a groove is sunk round the nave about a quarter of an inch in depth, into which a circular rim in the box is made to fit close, but with sufficient play for the wheel to revolve in it; the box opens in half, similar to a pair of handcuffs, and the axle of the carriage passing through it, the wheel is properly placed, and the box shut and fastened with a screw, keeping the rim firmly in the channel cut in the nave. As long as the axle of the carriage remains sound, these false axles and boxes bear no part of the weight of the carriage; but on a fracture taking place, the pressure is immediately transferred to the boxes supported by the false axles, and the wheel still keeps revolving in its place, held by the rim of the box in the groove of the nave.

Mining Journal.

* This is an expensive mode of attaining the same end, that has already been reached in this country, in a much more cheap and simple manner, by the well-tried contrivance known by the name of its ingenious inventor, as "*Kite's Safety Beam.*"—Com. Pub.